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ABSTRACT

This journal is intended to provide linguistics field workers with news, reviews, announcements, and articles that will stimulate interest in the field of linguistics. Each issue contains three to seven articles and reports, three to five book reviews, announcements relative to the field, and a list of books available for review. Feature articles in these issues include: (1) "And Where Now, Cecil?" (Geoffrey Hunt); (2) "The Role of Contextual Assumptions in Wh-Questions Containing the Particle /mc/ in Wee (Kru)" (Inge Egner); (3) "Speech-Led Versus Comprehension-Led Language Learning" (Eddie Arthur); (4) "The Role of Tone Sandhi in Tone Analysis" (John Daly); (5) "Linguistics as a Stepchild: A Diary Entry" (John Verhaar); (6) "What Can You Do With Findphone?" (David Bevan); (7) "Head Shift: A Diary Entry" (John Verhaar); (8) "Intra-Causal Movement as a Response to Case Summon" (Oza-mekuri Ndimele); (9) "Adverbial Clauses and Topicalization in Me'en" (Hans-George Will); (10) "Some Reflections on Formal Syntax" (David Weber); (11) "Implicatures" (Ernest W. Lee); (12) "Mirror-Image Reduplication in Amele" (John R. Roberts); and (13) "Response to Hohulin Review" (Regina Blass). (MDM)

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Coordinator's Corner

In developing the Glossary of Linguistics Terms for use in tagging analyzed texts, we encountered what others have found, namely that a clear distinction between pragmatic concepts and syntactic concepts was not possible to establish. There are issues that clearly seem to belong in the pragmatics, but the range of concepts between pragmatics and syntax blends so that no discreet dividing line is available.

It is not surprising, then, that argumentation processes used in linguistic analysis of syntactic structures will be tested on issues that seem to be in the pragmatics field, and that the solutions of pragmatics questions will affect those drawn in syntax. Is there enough common ground between pragmatics and syntax so that a single set of hypotheses and argumentation steps will suffice for both fields? We are glad to offer an interesting article dealing with such argumentation in this issue: Inge Egner's *The Role of Contextual Assumptions in WH-Questions Containing the Particle mɔ in Wɛɛ (Kru)*. Concepts like *warrant* that Egner uses come from jurisprudence rather than from conventional linguistics. Would you say that we already have adequate terminology without importing from fields akin to linguistics, such as Egner uses? (I'm not saying that she was the one who went to jurisprudence to find the term: others have done that.)

We invite your impressions of the linguistic argumentation used, as well as regarding the substantive content of the articles.

—Eugene Loos

And Where Now, CECIL?

Geoffrey Hunt

Eurasia Area Academic Computing Consultant

Many of the readers will be familiar with the SIL computer programs, *CECIL* and *Spectrum*. Together with an IBM-compatible computer and a *CECIL* box (which plugs into the parallel port of the computer), they can provide a cheap, portable, battery-operated phonetics laboratory. They are used worldwide by many linguists, both SIL members and others, and the pitch-extraction algorithm has received particular praise. They have been used to a much more limited extent by ethnomusicologists, and have even been used to identify two new species of Papuan frogs.

But where now? The two programs are essentially the same as those released with the first production *CECIL* boxes, which was during the first half of 1990. The vision of what *CECIL* might eventually do has not died—rather it has grown. In fact, development is continuing in both Britain and America. The purpose of this article is to let readers know what the present situation is, where we are going, and where we might be going. But it must be remembered that the whole thing is a research project, so please don't hold me to account if it does not go the way I say or predict.

The Way Ahead

All versions of *CECIL* up to the present version (1.2a) have been written by Philip Brassett[1], and I have recently received a new version from him. It is called *VGACECIL*; it is a beta test version, makes provision for VGA screens, has one known bug, and will probably not be distributed. The purpose of producing *VGACECIL* was to add the VGA facility, and to modularize the component parts of the program in a more efficient way, opening up the possibilities for further development.

In the next stage, hopefully finished by the end of January 1993, Philip plans to combine the two programs. This will be *CECIL*

version 2.0, and should contain an improved algorithm for producing spectrograms from utterance files. Any resulting spectrograms will be appended to the utterance file.

All this activity is really only consolidating what is already available to the linguist, but the next planned step will add significant capability to the program. This will be done by using a technique named *linear predictive coding* (LPC) to analyze utterances. LPC analyzes the vocal tract as if it were something like an organ pipe of varying diameters, producing resonant cavities and so modifying the original sound. We have not set a date by which this will be completed.

The mathematical for LPC is complex, and it can be approached in several ways, so there is going to be a need to experiment and see which approach is most beneficial. The normal approach to the mathematical of LPC has not allowed for the fact that, when the uvula is open, there is a second 'organ pipe' branching off. Martin Hosken, in Britain, has been working to provide the mathematical basis for adding LPC to *CECIL*.

In America at the University of Texas at Arlington (UTA), Prof. Jerry Edmondson has been asking for extra features to be added to *CECIL*. He wants LPC and he wants to be able to measure the *sound pressure level*, sometimes referred to as the oral pressure wave. This is important for some languages of southwest China, and some African languages, such as where breathy vowels are encountered. He works with Norris McKinney, who also has a longstanding interest in acoustic phonetics.

Meanwhile, things have also been happening at the JAARS Center in North Carolina. For a long while Terry Gibbs has been interested in the use of computers to study acoustic phonetic phenomena, and has been actively involved since 1985. After the original prototype *CECIL* boxes had been developed in Britain, Terry played a major role in designing the production boxes. Terry is now much more involved in software development and hopes that future *CECIL* boxes will be off-the-shelf commercial products.

Since September this year, Terry and Alec Epting, a former IBM employee specializing in digital signal processing, have been working

together to plan a second generation *CECIL*, using the best of what is already included in the present programs, and developing from there. Alec is trying to develop the mathematical theory of LPC to adequately handle nasalization. Between their work and the work going on in Britain, we hope to get a good implementation of LPC, and then we can try what we really want to do.

The Dream

Most of what I have spoken about so far have been technical matters relating to computer hardware and computer software. But the real dream concerns linguistic data. What can we hope to achieve in the automatic analysis of spoken text?

If we start with the linguistic description of a phone, let's say a *voiced labio-dental fricative with egressive lung air*, it points the way towards what is required. We need to determine whether there is voicing or not, what the point of articulation is, the type of sound, and the air mechanism used. As regards the phone's larger environment, we need to look at loudness, frequency of voicing and duration. Some features, such as nasalization, may fall into either the description of the phone itself or its environment.

In the table below I list the ways in which we can measure aspects of a sound, either measurements that we can already do or measurements that we think we will be able to do:

Measurement	**Information revealed**	**Status**
Signal amplitude	Loudness, some phone junctions	Easy — okay
Timing	Duration	Easy — okay
Frequency of voicing	Pitch, intonation patterns	Tricky — almost okay
Spectrogram	Type of some sounds, phone junctions, formants	Difficult — being improved
Sound pressure level	Air mechanism, breathy sounds	Needs research
Linear predictive coding (LPC)	Point of articulation, nasalization, type of sound, phone junctions	Needs research

Measurement of the *sound pressure level* has traditionally been done by fixing a sensor over the mouth of the speaker. This is not

satisfactory, in my opinion, because it could distort the speech. It may be possible to achieve the same by the analysis of the very low frequency changes associated with a sound, but that is a matter for experimentation.

If linguists want to make the most of these developments, they will need high quality tape recorders. And, if they want to study the *sound pressure level*, even a high quality tape recorder might not be good enough. It may be possible, though, to use a new type of *CECIL* box to directly record such sounds.

Then, if we are able to make all these measurements accurately, we may have all the information needed to automatically mark the appropriate *phonetic parameters*. However, this does **not** mean that we would be in a position to produce a good *phonetic transcription*. We may have all the information we need, but getting that information into a form that is useful may be quite difficult. It will require considerable experimentation, and success cannot be guaranteed, because human speech is very complex. Some members of the team are more confident than others.

Whatever we end up with, we will certainly have a lot more information than is normally available in phonetic notation. For example, there will be much more precision in recording pitch and duration, therefore we will have to devise a new method of recording these phonetic parameters. The work of the generative phonologists is likely to be useful here.

Some features will not lend themselves to convenient analysis in terms of parameters. The frequency of voicing is one such feature, except where voicing ceases. It will probably have to be recorded in considerable detail, separated from the main phonetic parameters.

It may be possible to do some limited *phonological analysis* with the resulting phonetic parameters. Again we must experiment to find out what is possible.

Steven Bird, an Australian doing post-doctoral research at Edinburgh University in Britain, is researching a project related to the phonological analysis of features often overlooked in conventional analyses. To do this, he requires 5000 words of accurately transcribed

phonetic text. This is keyboarded and analyzed by computer programs that have been developed there in Edinburgh. The problem, of course, is that it is very difficult to provide 5000 words of accurately transcribed phonetic text. If only we could automatically reduce 40 minutes of recorded speech to phonetic parameters, such computer processing would be more realistic.

Final Comments

Of course, this is only a dream. But the world does need its dreamers. Some of us dreamed about *CECIL* and it became a reality. Should we not let the dream continue? It remains to be seen how successful we will be. The only funds we have are the limited funds that the International Computer Services at JAARS can make available. But there is plenty of enthusiasm, and the success of *CECIL* encourages us to believe that we can achieve something even more significant.

So we will persevere, but don't hold your breath.

Footnote

[1] I have not indicated the doctoral degrees of several people mentioned. ■

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The Role of Contextual Assumptions in WH-Questions Containing the Particle *mɔ* in Wɛɛ (Kru)

Inge Egner

SIL Abidjan

In the following paper[1] I suggest a pragmatic approach to the analysis of a conversational particle in Wɛɛ.[2] That is to say that I shall talk about the contextual factors governing the occurrence of this particle. By context, I do not mean the linguistic context, which, following Halliday, is generally called cotext. Nor do I mean the real world, i.e., the situational context. But the notion of context I am using here refers to background assumptions held by the speaker and the hearer in verbal communication. I hope to show that pragmatic analysis can be done in a systematic and methodical way, even though pragmatics as a discipline cannot yet claim as solid a methodological foundation as phonology or syntax.

I have not been able as yet to analyze all the different uses of the *mɔ* particle, which occurs in questions as well as in declarative utterances, imperatives and certain subordinate clauses such as conditional clauses. However, whereas in global questions and declarative utterances *mɔ* always seems to occur in combination with other particles, most of which are also still unanalyzed, it is in WH-questions only that it has been found to appear on its own. Such questions therefore are a reasonable starting point for the analysis of *mɔ*.

I

It was through the following question-answer sequence occurring in a natural Wɛɛ conversation that I first became interested in studying *mɔ* a little more closely:

(1) R: *dɛɛdɛ* *aa* *'wlv* *'bɪɪa'* *mɔ* *ɛ-*[3]
what exactly you (F)-INAC leave clearing-NOM CV QWH[4]
'So what exactly have you just cleared, then?'

F: *''ɪɪn–, 'ma 'wlvɛ 'doo 'di i 'bɪɪa'*
no I-INAC leave-DECL plot inside PC clearing
'No, I have just cleared my plot for building.'

What strikes most in this sequence is the 'no' introducing F's answer to R's question. 'No' seems a little strange as part of an answer to a WH-question, at any rate in English and other Indo-European languages. However, checking with my Wɛɛ source made it clear that F's answer is perfectly acceptable.

That F's 'no' cannot be understood as a refusal to answer R's question is already obvious from the fact that the second part of his utterance does give R the requested information ('I have just cleared my plot for building'). The only other way in which F's 'no' can make sense, then, is to say that it addresses something implied by R's question, some kind of assumption underlying it. In order to account for the well-formedness of the sequence, we shall thus have to retrieve this assumption. Since background assumptions are activated in specific speech situations, we need to place the sequence we are studying into its situation.

F has just arrived in R's compound, machete in hand. After the traditional greetings, R starts a conversation by asking F the routine opening question 'Where are you coming from?' To which F answers, 'I've just done clearing work by the roadside.' Unless specified otherwise, clearing work normally refers to clearing a field before sowing. However, the conversation takes place in the midst of dry season, when that kind of work is unlikely to be done. Therefore speaker R feels entitled to ask F a question about the type of clearing work he has done, i.e., his request for information is warranted.

Claiming that the occurrence of the particle *mɔ* in R's question has something to do with this, I suggest the following **hypothesis** (H1) about its meaning:

H1: *mɔ* in a WH-question signals that the request for information is warranted.

The warrant of an utterance can be made explicit by a subordinate clause introduced by the connective *since* as in the following **paraphrase** (P1) of R's question in (1):

(P1) (I am asking you what kind of clearing work you have done, since we are in dry season)

However, such a warrant is only valid by virtue of an underlying **contextual assumption** (CA) held by the speaker and acceptable to the hearer. For R's question in (1) we might formulate this assumption as follows:

(CA1) [Clearing work on fields is not done in dry season]

This means that the particle *mɔ́* has ultimately to do with contextual assumptions, which leads me to reformulate hypothesis H1 as follows:

H2: *mɔ́* in a WH-question signals to the hearer that the speaker is calling upon a contextual assumption which warrants his request for information.

Using this hypothesis, the explanation of speaker F's 'no' in example (1) would seem to be that F, while giving speaker R the information he has asked for, refuses to accept R's question as being warranted by assumption (CA1) which is of course also accessible to him. In F's understanding (and a remark of R's later in the conversation confirms this), R has failed to take into account an important piece of background information, which F could have expected R to remember.

In fact, a government officer's visit to the village was announced for the following day. For several days including that very morning, the villagers had been publicly reminded at daybreak by the town caller that they were supposed to prepare the village for the officer's visit by clearing their compounds as well as any weedy bits of ground, especially next to the main road. Even though R does not participate in any such work, being too old, he could nevertheless have inferred, upon hearing that F had just done clearing work, that F was not talking about field work but about some other type of clearing work related to next day's event, viz., the clearing of the plot which he intends to develop.

II

Hypothesis H2 seems to be borne out by examples (2) and (3):

(2) B and Z talk about a new house that B has gone to inspect the day before with friends, in order to see if he would rent it. Having asked where the house is situated and who the owner is, Z goes on to ask the following question:

Z1: *ao mua 'de ɛ 'di -kwiie''- 'dee,*
you(PL) go-SUB there it inside yesterday DEPQ
'Since you went inside yesterday,

B1: *ɪɪn*
yes
'Yes'

Z2: *ka 'e nɪ mɔ ɛ-*
how there be CV QWH
what is it like, then?'

What warrants Z's question in Z2 is explicit in Z1. The paraphrase for Z2 thus reads as follows:

(P2) (I am asking you what it is like inside, since you went in yesterday)

The contextual assumption underlying Z's question could be the following one:

(CA2) [Before renting a house, one takes a look at its inside]

In the following example, the underlying contextual assumption warranting the request for information is retrievable in a similar way through the cotext:

(3) B and Z are talking about their respective field work. When asked by B where he is up to with his work, Z answers that he is through with clearing all his fields. Given the fact that Z has just mentioned his return from the capital, B feels entitled to infer that Z has had help and asks accordingly.

B: *-ɔ -bɪɪɛ aa' dɪ mɔ 'pepe ɛ-*
who clear your(F) things CV well QWH
'So who cleared your fields for you, then?'

The paraphrase of B's utterance reads like this:

(P3) (I am asking you who cleared your fields for you, since you have been away)

The contextual assumption underlying (3) could be formulated as follows:

(CA3) [A farmer who is away at field working time falls behind with his work]

Unlike examples (2) and (3), where the assumption warranting the request for information could be directly retrieved through the cotext, the following example gives no such cotextual clue. The underlying contextual assumption will thus have to be established solely on the basis of the extralinguistic situation.

(4) A group of bush farmers returning from their fields meet a group of white people including myself on the bush road and ask them:

-ta aoa 'wlv mɔ ɛ-
where you(PL)-INAC leave CV QWH
'So where have you come from, then?'

The farmers were obviously surprised to bump into us in an area where white people would not normally visit. The paraphrase of their question is thus:

(P4) (We are asking you where you have come from, since white people do not normally visit out here)

The contextual assumption underlying (4) can be formulated like this:

(CA4) [White people do not normally visit out in the bush]

Interestingly enough, the question in (4) contrasts with the following question, which is the routine question for opening a conversation.

(5) *-ta aoa 'wlv ɛ-*
where you(PL)-INAC leave QWH
'Where have you come from?'

Given the routine nature of question (5), the speaker would not normally feel the need for warranting his utterance. One would therefore expect that the particle *mɔ* cannot be used in (5), which is confirmed to be the case. On the other hand, the presence of *mɔ* in (4) indicates that the farmers do precisely not intend their question as the normal routine question.

III

Pursuing the analysis of *mɔ*, I proceeded to ask my informant to make explicit the meaning of the utterances containing the particle *mɔ* through paraphrases in the Wɛɛ language. He then gave me the following ones for examples (3) and (4):

(P3') *-ɔ -bɪɪɛ aa' dɪ mɔ 'pepe ɛ-*
who clear your (F) things CV well QWH
'Who did the clearing of your fields for you,

-wɛɛ' ɪa' 'bɪɪa -kwɛ ɛ-
then their (NH) clearing be finished QWH
so that it is finished?'

(P4') *-ta aoa 'wlv mɔ -wɛɛ' ao 'nynɪ -ta*
where you (PL)-INAC leave CV then you arrive here
'Where have you come from that you arrive out here

'kwla 'di ɛ-
bush inside QWH
in the bush?'

In these Wɛɛ paraphrases, the syntactic constituent making explicit the assumption warranting the request for information is a dependent clause introduced by the connective *-wɛɛ'*. This connective is used to express a number of different interpropositional relations such as time ('then'), purpose ('in order to'), and opposition ('but', 'however').[5]

Since the temporal relation between the two clauses in (P3') and (P4') is trivial, it seems that it can be ruled out right away for those utterances.

The relation of purpose can be said to hold in (P3'), where the main predicate has past meaning. However, this relation does not hold in (P4'), since the unaccomplished aspect in the main clause would call for the modal auxiliary *'je* AUXPOT in the dependent clause. Moreover, if a purpose relation were intended, the connective *wɛɛ'* would introduce the dependent clause only optionally, as shown in the following example:

(6) ɔɔ muɛ -gbei˜ (-wɛɛ') ɔ 'je dɪ 'bɪɪ-
he-INAC go-DECL encampment (then) he AUXPOT things clear
'He is going to the encampment in order to do clearing work.'

For questions (3) and (4) this leaves us with the rather vague notion of opposition, which I shall now examine more closely.

As has already been pointed out in section II, both (3) and (4) are uttered in a speech situation which involves facts contrary to the expectation of the speakers: Z, even though having been away, is already done with his field work; the farmers, even though deep in the bush, meet a group of white people. It is important to note that a speaker perceives a situation as unexpected because of the contextual assumption he holds.

In contrast to questions (3) and (4), the question of example (2) ('Since you went in yesterday, what is it like inside, then?'), does not convey the idea of counterexpectation. On the contrary, B having just informed Z that he had gone to see the house in question the day before, Z can expect him to know what it is like inside.

The fact that Z considers the situation to be in accordance with his assumption is also expressed in the way he encodes it in his question. Rather than by a sequential clause introduced by the connective *-wɛɛ'*, which we had in the Wɛɛ paraphrases (P3') and (P4'), it is expressed by a subordinate clause preceding the question.

IV

It is syntactically possible to give a paraphrase with the sequential *-wɛɛ'* clause for question Z2 in example (2), as is shown by the following utterance:

(P2') *ka ɛ 'di ni mɔ -wɛɛ' ao mu 'e ɛ-*
how it inside be MP then you(PL) go there QWH
'What is it like inside, so that you went in?'

However, paraphrase (P2') does not have the same meaning as question Z2 in (2). Indeed, whereas in Z2 the speaker implies that B's looking at the inside of the house has nothing surprising, paraphrase (P2') conveys that B acted in an unexpected way. Clearly, therefore, Z's contextual assumption in uttering (P2') cannot be the same as for (2). For (P2') one might imagine a situation like the following: The houses among which B is going to choose all have an identical plan. Z knows that B has already looked inside one of them. So when B tells him that he went inside another one, Z feels entitled to ask (P2'). The contextual assumption warranting his question can be formulated at several levels of generality, like any of the CAs formulated above, by the way. A more general formulation of the assumption warranting (P2') might read like this:

(CA2') [One does not inspect an object that one knows to be identical with another one already inspected]

By appealing to a CA like (CA2'), Z claims B's behavior to be contrary to the expectation arising from this assumption. Z feels therefore entitled to ask the question in (P2').

Thus the questions containing *mɔ* in (2) and (2') illustrate the fact that the contextual assumption warranting a speaker's request for information may or may not match the speech situation. I shall hereafter distinguish these two cases by talking about a validated or an unvalidated CA.

From among the examples given so far, only the CA called upon in utterance (2) is validated. In the other utterances, such is not the case: Thus in (1), (CA1) is invalidated by F's talking about doing clearing work in the midst of dry season. So is (CA3) for question

(3), since Z has said that he has finished his field work in spite of having been away traveling. Finally, (CA4) is invalidated, since the farmers in (4) meet a group of white people, even though being far out in the bush.

Even though our examples suggest that *mɔ* is more frequently used to call upon an invalidated CA, speakers also use it for calling upon a validated CA. The idea of contra-expectation cannot be said, then, to be due to the mere presence of *mɔ* in the request utterance, as has especially become evident from utterances (2) and (P2'). Rather, *mɔ* only testifies to the fact that a CA is being called upon by the speaker, but does not say anything about the way in which it warrants the utterance, i.e. whether it is validated or not in the speech situation. To establish this relationship is part of the interpretation task the hearer has to accomplish by attributing the relevant contextual assumptions to the speaker's utterance.[6]

V

Now, as a consequence of whether the speaker calls upon a validated or an invalidated assumption, his attitude toward his utterance will be different. That is to say that the illocutionary value of a request for information containing *mɔ* can be expected to be different depending on the validation or the non-validation of the contextual assumption called upon.

Compare again the question Z2 in (2) and the Wɛɛ paraphrase (P2'). Z2, in which CA2 is clearly validated, appears as a request for further information about the house in question. By contrast, (P2') makes explicit the invalidated character of (CA2') and requests the hearer to explain why he went into that particular house, given the fact that they were all identical and that he had already looked at another one before. In other words, (P2') has to be interpreted as a request for an explanation.

To the extent, then, that utterance (3) calls upon (CA3), which is an invalidated CA, it has also to be interpreted as a request for an explanation. Similarly, utterance (4), by virtue of the invalidated assumption (CA4), is a request for an explanation rather than a request for information.

In the same way, it makes more sense to consider utterance (1) as a request for an explanation than as a request for information. Indeed, clearing fields not normally being done during dry season, the speaker feels entitled to request that the hearer explain to him why he has done clearing work at this time.

We can now make the following hypothesis about the interpretation of WH-questions containing the particle *mɔ*.

H3: A WH-question containing the particle *mɔ* is to be interpreted as a request for information if the CA warranting the utterance is validated, and as a request for an explanation if the CA is invalidated in the speech situation.

VI

However, WH-questions containing *mɔ* can still be interpreted in a third way. Consider the following example:

(7) *dɛɛdɛ ɔ jri mɔ ɛ-*
what he steal CV QWH
'So what has he stolen, then?'

This question could be uttered in a situation where the speaker learns about someone being in prison for theft. It is to be interpreted as a request for information if the CA called upon the speaker is validated, which would for instance be the case with the following one:

(CA7) [A thief must have stolen something significant to be put in prison]

However, the same question is to be interpreted as a request for an explanation, if for example the thief is a young boy and the speaker finding the punishment excessive asks what kind of theft justifies the imprisonment. The CA he is then calling upon by way of the particle *mɔ* could be the following:

(CA7') [A young thief should not be put in prison]

Finally, question (7) can be uttered by someone who has had his sheep stolen and hears about a thief being caught. Hoping that the

thief in question is identical with the one who stole his sheep, he expects the response to his question would confirm his hope. His question could thus be paraphrased like this:

(P7) (What has he stolen if it isn't my sheep?)

The underlying CA could in this case be the following:

(CA7'') [Any thief caught could be the one who stole my sheep]

In other words, the question is to be interpreted as a request for confirmation. Its indirectness is due to the fact that the question really aims at validating the underlying assumption, which is done through the piece of information asked for.

In the following example, the speaker knows that X's wife has up to now always given birth to girls. When hearing that she has again had a baby, he expresses his expectation that it is a girl through question (8), which is thus to be interpreted as a request for conformation of his assumption.

(8) *dɛɛdɛ v 'wlv mɔ ɛ–?*
what exactly she give birth CV QWH
'So what did she give birth to, then?'

Like example (4), which without the particle *mɔ* is routine question (5) for opening conversations, leaving out the particle *mɔ* in question (8) makes it part of the sequence by which a birth is traditionally announced. More generally, one might even say that the routine nature of an utterance is characterized by the absence of *mɔ* and of speaker assumptions called upon by this particle.

VII

In analyzing the meaning of the particle *mɔ*, I started from the working hypothesis that it marks a WH-question as a warranted request for information. After making the warrant explicit by an English paraphrase, I have tried to formulate the underlying contextual assumption. Wɛɛ paraphrases revealed that some questions conveyed an idea of contra-expectation, which was due to the fact that the CA called upon by the speaker did not match the

speech situation. Thus appeared a systematic relationship between a CA, a speech situation and the illocutionary force of a WH-question containing the particle *mɔ*, which can be interpreted as a request for information, a request for an explanation or a request for confirmation.

NOTES

1 This paper was originally read at the 19th WALS congress, Accra, 1st to 6th April 1990. I am grateful to Stephen Levinsohn for helpful comments on the manuscript.

2 Wɛɛ is a Kru language spoken on either side of the Libero-Ivorian border. The data used in this paper are in the Tao dialect, which belongs to a Wɛɛ dialect complex spoken in the southwest of Côte d'Ivoire and commonly referred to as the Wobe language.

3 In Wobe orthography, apostrophe, quotation marks, and hyphen are used to represent pitch level: ' (apostrophe) stands for high tone, '' (quotation marks) for very high tone, and - (hyphen) for low tone; middle tone remains unmarked. Tone signs at the end of a word indicate the pitch to which a tone glide falls or rises. Thus in *–paa''* *'cassava'* the tone rises from low to very high, and in *''maa'* 'forget-DECL', it falls from very high to high. However, hyphen at the end of a word indicates the falling of the tone to mid within an utterance and to very low in isolation or at the end of an utterance, e.g., *''ɪɪn–* 'no'. In *'bɪɪa'* 'clearing-NOM', the nominalizer *-a'* (toneglide from mid to high) is added to the verb *'bɪɪɛ–* 'clear' (tone glide high-falling). The resulting tone sequence is high-mid-high.

4 In the word-by-word translations of the examples, the following abbreviations are used for grammatical features of the Wɛɛ language:

AUXPOT	auxiliary of the potential mood
CV	conversational particle
DECL	declarative suffix
DEP	dependency marker
DEPQ	dependency marker before interrogative main clause
F	feminine gender
INAC	inaccomplished aspect
PC	pro-complement
PL	plural
QWH	question marker for WH-questions
SUB	subordination suffix

For further detail, see EGNER (1989).

5 For more detail about the uses of *wɛɛ'*, see paragraphs 296–299, 301, and 304–308 of EGNER (1989a).

[6] It would no doubt have been interesting and fruitful to exploit Sperber and Wilson's (1986) Relevance Theory to a greater extent in this discussion of *mɔ*. Unfortunately, time has not allowed me to look at more data necessary for a treatment within RT. Blass's (1990) description of the *ré* and *sié* particles in Sissala, which both seem to have a number of uses in common with the *mɔ* particle in Wobé, illustrates a promising path into future research of this kind.

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Speech-Led Versus Comprehension-Led Language Learning

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> *From the very beginning... the learner should talk, talk, talk... (Healey)*
>
> *...it has been found that a 'silent period' at the beginning of the learning process, during which the learner simply listens to the new language and is not mad to produce it, greatly enhances the speed and quality of learning (Dulay, Burt and Krashen).*

What is the language learner to do in the face of conflicting advice like this? There are numerous guides to language learning, offering many forms of advice and frequently contradicting each other. What is worse, the terminology used is often that of the psychologist or classroom teacher and not that of the field linguist. In this paper I will attempt to give a brief overview of some issues in language learning and to do so, I will adopt my own terminology. Language learning will be referred to as being either *Speech-led* or *Comprehension-led*, depending on which activity the learner concentrates. These terms do roughly overlap with more usual descriptions of language learning, but I will continue to use them because of their immediate relevance to the task of field language learning.

Speech-led

One of the best known speech-led schemes of language learning is the 'LAMP method', taken from the book *Language Acquisition Made Practical (LAMP)* by Brewster and Brewster. This method of language learning revolves around the daily learning cycle of

preparing and practicing a text, saying the text to as many as fifty people in the community and then evaluating the day's progress.

By the end of the second day of language learning, the learner could express something like this:

> Hello, how are you?
>
> I want to learn your language.
>
> This is my second day here.
>
> This is all I can say so far.
>
> I'll be seeing you.
>
> Good-bye. (LAMP p. 8)

Each day the learner elicits a text and memorizes it, drilling any difficult aspects of pronunciation and grammar, but the major focus of each day's activity is the communication phase, where the learner goes for a walk, repeating her text to as many people as possible. *The learner gets a little but uses it a lot.* It is this repetition of material which is seen as the key to mastery of the language. Sufficient repetition (50-100 times) of a memorized text will bring the learner to the point where she can use the material spontaneously and recognize it when it is addressed to her. Although some comprehension activities are built into each cycle, it is the learner's production of language which is seen as paramount, not comprehension.

> Of course, you will probably not understand most of what they say to you. But don't get paranoid. (LAMP p. 26)

LAMP insists that all text material and drills should be culturally relevant, interesting and appropriate to the stage that the learner has reached and provides many useful ideas for language learning. The inventiveness of LAMP coupled with the rigor of the daily learning cycle are certainly a great help to the language learner and this method has been used successfully both within SIL and elsewhere (Brewster and Brewster 1981).

Other descriptions of speech-led methods of language learning can be found in books by Larson and Smalley, and by Healey, as well as in various SIL language learning handbooks (cf. Burgess and Andersson).

Comprehension Driven

Comprehension driven language learning is a more recent idea than the speech-led model. There are no guide books along the lines of LAMP to aid the learner, although there are some publications which suggest methods to follow (Kindell, Thomson, and others). Rather than memorize and repeat texts to build up a command of the language, the learner seeks, first, to understand language directed towards her. The learner uses various techniques to ensure a flow of language input at an appropriate level. Just as the speech-led model assumes that repeatedly using a phrase will allow the learner to understand it on hearing it, the comprehension-led model assumes that if the learner fully understands a piece of language, she will eventually internalize it as a part of the bank from which she will draw her own language production. To use Dwight Gradin's phrase, *language must be got in, before it can come out.*

Theoretical Basis

Although the terms speech-led and comprehension-led language learning are essentially practical, referring to what the learner does, there is a theoretical basis underlying each model which I would briefly like to examine.

The theory lying behind speech-led language learning is essentially behaviorist. Language is seen as a series of habits formed in response to stimuli. Behaviorist psychologists saw first language (L1) learning as being speech-led—children mastering their mother tongue by imitating utterances produced by adults and having their speech either rewarded or corrected. In this way children were thought to build up a knowledge of the patterns that comprise the language they are learning. For example, a child hearing the word *drink* repeats it without understanding what it means and is immediately offered a glass of water. This scenario is repeated over time, the child saying *drink* and the adult proffering water and gradually the child comes to associate the utterance with the offer of a drink. However, if the child says *tink* there is no parental reaction and the child soon drops *tink* from her verbal repertoire. In the one case a habit is built up through reinforcement while in the second case the word is not reinforced and falls out of use.

The speech-led model sees second language learning in the same way: repetition and reinforcement of utterances allowing the learner to form new language habits. The new language needs to be broken down into manageable pieces which by repeated drilling and practice become automatic to the learner.

> Language learning involves acquiring a set of automatic habits that will enable us to communicate successfully... Each feature must be thoroughly mastered before any real victory is possible (Healey, p. 285).

The more modern behaviorist language learning guides such as LAMP and Healey do see a need for the learner to understand the meaning of the material she learns. Nonetheless, it is the repetition of that material which is seen as the most important factor. This is clearly illustrated by the daily learning cycle in LAMP, with various forms of drills in the practice phase and then the 30-50 repetitions of a text in the communication phase.

Two important factors in speech-led language learning are the attitude to learner's errors and the effect of the learner's mother tongue.

Errors are viewed as a serious problem in this model of language learning. The problem is seen to be that if the learner produces speech containing errors and the errors are not corrected, then these errors will, in effect, be reinforced and become habits, and so form part of the learner's ongoing speech. Thus from the very outset the learner is encouraged to drill and practice so as to produce perfectly formed utterances.

The major source of error is thought to be the learner's own mother tongue. Linguistic habits learned in childhood, then practiced and reinforced over many years, are seen as a block to acquiring the new habits required for learning a second language.

> The grammatical apparatus programmed into the mind as the first language interferes with the smooth acquisition of the second (Ellis, p. 22).

So in many ways second language acquisition is seen as a process of overcoming the habits formed in learning a first language and replacing them with a new set of habits.

> The process of second language acquisition is often characterised in popular opinion as that of overcoming the effects of L1, of slowly replacing the features of the L1 that intrude into the L2 with those of the target language and so of approximating ever closer to native-speaker speech (Corder, 1981).

The mother tongue is not solely seen as the villain of the piece. While there are areas in which mother tongue habits hinder the learner, it is equally possible that a feature from the L1 may be present in the L2 providing the learner with ready learned features.

For instance an L1 English speaker would find the French phrase *j'ai trente ans* (I have thirty years) difficult and may well end up making an error, because it does not fit his established English habits, whereas the corresponding German phrase *Ich bin dreissig Jahre alt* (I am thirty years old) shows the same construction as the English and should be easily learned. Some authors refer to negative and positive transfer when errors are produced, or avoided because of carry-over from L1.

If the mother tongue is the major source of second language learner errors, then it ought to be possible to predict the areas where learners would have difficulty by a careful comparison of the two languages, a process referred to as Contrastive Analysis.

> Learning difficulties arise at the points where the structure of your own language differs from that of the new language—the target language, There is said to be **mismatch** between the mother tongue and the target language at these points. You need to recognise what the mismatches are between your own language and the new one. You have then located the trouble spots in your language learning and must tackle these systematically with appropriate drills (Burgess and Andersson, p. 59).

There are a number of serious problems associated with the behaviourist basis of speech-led language learning which we need to briefly examine. In first language studies it was pointed out (Chomsky, 1959) that a behaviourist model could not account for the rich, creative use of language that learners are capable of. In addition the premiss that children first learn to speak by imitating adult utterances cannot explain why early learners typically combine words in a way which they could never have heard from an adult.

Research has also shown that the idea that errors in L2 learning are due mainly to habits carried over from the L1 (contrastive analysis), though outwardly sensible and obvious, does not actually hold true in practice. Dulay and Burt found that among adult Spanish speakers learning English only 3 percent of errors could be attributed to their mother tongue. Although Ellis suggests a much higher proportion (33%) of learner errors can be attributed to the effect of their first language, this is still much less than half.

In addition to this, learners do not always seem to benefit from habits from their mother tongue which would help them learn the new language. For instance, adult Spanish speaking learners of English would often not use the English plural morpheme *s*, despite the fact that Spanish has the same feature (Dulay and Burt, p. 98).

However, although the evidence seems to be that learner's grammatical errors are not derived from their mother tongue, it does seem that many (although not all) phonological errors do reflect the learner's L1. In reality this is fairly obvious. We can generally spot a Frenchman speaking English (or an Englishman French for that matter) by his accent.

The theoretical basis for comprehension-led language learning is somewhat different from that which we have discussed so far. The learner acquires language by hearing the language spoken (input) and internally organizing the data into a system. Having processed language input, the learner will be able to produce new and unique utterances. Stephen Krashen suggests that a learner acquires[1] a new grammatical structure when she understands input containing that structure.

> Humans acquire language in only one way—by understanding messages or by receiving 'comprehensible input' (Krashen, 1985).

For example, a learner with no knowledge of the past tense may be able to understand input referring to past action because of the presence of previously acquired temporal phrases such as 'yesterday'. She would then unconsciously assimilate the changes in the verb phrase associated with past action, building up a knowledge of their structure and eventually producing new utterances containing the structures, herself. No one is entirely sure how the system of the new language is built up by the brain and there are a number of different

theories to explain the process (see Mclaughlin). One thing that does seem to happen is that the learner adopts a process of hypothesis testing. The learner erects a hypothesis about a grammatical structure and produces utterances in accordance with it. Reaction to the utterance may then cause her to adopt a new hypothesis or accept her current hypothesis as true.

Mother	Did Billy have his egg cut up for him at breakfast?
Child	Yes, I showeds him.
Mother	You what?
Child	I showed him.
Mother	You showed him?
Child	I seed him.
Mother	Ah, you saw him.
Child	Yes, I saw him.

Here the child within a short exchange appears to have tested three hypotheses: one relating to the concord of subject and verb in a past tense, another about the meaning of show and see and a third about the form of the irregular past tense of see. It only remains to be pointed out that if the child had said **I saw him** immediately we would have no means of knowing whether he had merely repeated a model sentence or had already learned the three rules just mentioned. (Corder)

Errors, then, are no longer seen as the bugbear of the learner, but rather, as a positive thing, showing evidence of the process of hypothesis testing. The learner is not expected to produce near perfect utterances from the start, as in the speech-led model, but rather will proceed by a series of stages. The learner is said to possess an 'Interlanguage', an intermediary form which should be constantly changing as it approximates more and more to the new language. Of course, few adult learners reach native-speaker proficiency in a new language. The comprehension-led model does not regard this as being due to uncorrected errors becoming reinforced as habits. Rather the learner is thought to reach a point at which her ability in the new language is adequate for her purposes and then social and emotional (affective) factors, such as lowering of motivation, take over preventing her from making any further

progress and the learner's interlanguage becomes 'fossilized' at an intermediate point.

Practical Applications

Of course most language learners are more interested in methodology than in theory; they want to know 'how to learn a language'—preferably by some easy, fail-safe method. The problem is that, despite all our vain hopes, there isn't an easy way to learn a language (shame), so what should the learner do?

The great advantage of a book such as LAMP is the structured program which it presents. The learner is provided with an overall scheme and lots of good, creative ideas to add variety. However, as we have seen, speech-led methods such as LAMP are based on a behaviourist model of language learning, which is not an adequate description of the learning process. By comparison, comprehension-led methods of language learning have a better theoretical basis, but no good, practical program has been made widely available. In closing I would like to examine various practical aspects of language learning and see how they might be integrated into a comprehension-led method.

Socio – Affective Factors

> The term affective refers to emotions, attitudes, motivations, and values. It is impossible to overstate the importance of the affective factors influencing language learning (Oxford, p. 140).

To the learner in the field affective factors are of primary importance. The swings of mood from high to low, a feeling of failure, and the times when motivation is completely absent are regular features of life for most learners.

I believe that this is one area in which LAMP is very weak; to those of us who are introverts the insistence that we speak to thirty or more people a day is a very real cause of stress. Leaving aside the cultural difficulty of simply repeating a text and moving on rather than sitting together to exchange protracted greetings, many learners are overwhelmed by the idea of talking to so many people. Failure to learn the daily text and successfully repeat it can lead to learners

feeling guilty and becoming discouraged which can, in turn, make learning the next day's text even harder. Marjorie Foyle suggests that LAMP type methods of language learning are an important source of stress among missionaries.

The importance of affective factors in language learning has been taken up by a number of authors (see Brown). Most language learners are aware that affective factors can cause them to receive less input (staying in the office to classify data rather than mixing with people—we've all done it!) but they are less aware that these same factors also reduce their ability to process the input that they do receive. This concept is referred to as the affective filter (Dulay, et al). When the filter is high the learner is unable to process the information that she is receiving so, although she may be in a good language learning situation, she may not be able to benefit from it because of her physical or emotional state. Poor physical health, depression, fatigue, loneliness, and a host of other factors can all lead to a high filter and the learner not being able to make the best of her situation.

So, if a learner is to benefit from a comprehension-led model of language learning, attention must be paid to affective factors. It is unreasonable to expect language learning to be entirely stress free, but steps should be taken to lower the stress level as much as possible. In some ways a comprehension-led method of language learning is intrinsically less stressful than a speech-led method.

> An approach which stresses the development of the receptive skills (particularly listening) before the productive skills may have much to offer the older learner (Schleppergrell).

We have already noted the difficulty faced by the speech-led learner of having to learn and repeat a daily text. These difficulties are at their greatest early on in the language learning program, when the learner is not only concerned with the language, but may also have to build a house, meet lots of new people, and work her way through culture stress. To be sure, culture stress, house building and the rest, will always be present whatever method of learning is adopted, but within a comprehension-led program the learner will not be faced with the extra burden of having to repeat a text to forty or so people each day. The learner could concentrate on building a receptive

vocabulary using total physical response and other comprehension building activities. She could also spend lengthy periods with local people, listening to the language without feeling any pressure to speak or recite a text. In fact, sitting and watching and listening is a far more culturally appropriate activity in West Africa than saying a few words and then moving on. An adult learner would certainly need to memorize a few useful sayings such as greetings, leave takings, and requests for clarifications, but these can be used sparingly and the learner should feel no compulsion to speak the language until she is ready. This period when the learner is under very little pressure to produce the language not only reduces the stress factor it also allows the learner's cognitive apparatus to process the language. Being compelled to produce language too early is seen by some authors as an important source of problems in language learning (Krashen, 1982).

One suggestion is that the learner should not initially locate in the language area but somewhere close to it, then working with an intermediary person, someone from the target language group with a knowledge of the learner's culture, the learner uses comprehension-led techniques to build up a knowledge of the language before moving into the language area itself. I can see great advantages in this sort of approach, as the learner could take the first steps in language learning while living in the relatively low-stress environment of a town or mission station before moving out to 'the village'. Once in the village, having a basic knowledge of the language would help the learner adapt more smoothly into the new culture and situation. However, this begs a lot of questions regarding the role of the translator. Would 'bonding' be possible in this situation? Is 'bonding' a useful concept in our work anyway? These questions are of crucial importance to the language learner (see Hill, 1990a).

Affective factors are important throughout the language learning period and learners need to be made aware of strategies for managing their emotions and attitudes; both Healey and Oxford have a number of useful suggestions that could be incorporated into any learner's program.

Optimizing Input

If the learner's focus is on understanding the language then a large slice of her effort needs to be devoted to obtaining a flow of language input at a level which is appropriate to her. The learner needs to be able to control the flow of input in two areas, in informal conversation with native speakers and in the formal language learning sessions with a language helper. In informal situations the learner is very often completely overwhelmed by the amount of language which she hears and as a result she is unable to benefit from the exposure she is receiving. Rebel Oxford lists various strategies which are of help to the learner in this sort of situation. By taking an active part in conversation, the more advanced learner can request clarification or repetition and slow down the flow of input. In situations where intervention is inappropriate, such as village meetings or church, the learner may focus on one particular aspect or topic of the speech. For instance, in church a new learner may simply listen out for the word 'God' and not pay any attention to the rest of the sermon.

In the more formal situation controlling the flow of input is easier, but the learner must avoid boredom, both for herself and her language helper. Imaginative games can be of great help and can be varied so as to keep both parties interested.

The new language learner requires a lot of contextual clues in order to understand and make use of input, whereas the more advanced learner is much less dependent on the context. Greg Thomson defines four stages in the learner's progress, each with appropriate activities designed to provide the correct level of input for the learner.

Text based work can ensure a good supply of appropriate language input. After some sort of communal activity (a trip to the fields, a visit to another village), the language helper can be asked to record a brief description of what happened. The learner already understands the gist of what is being said as she participated in it, and with repeated listening will develop a knowledge of the syntax and lexicon of the text. One great advantage of this sort of approach is that the language recorded will be natural, containing grammatical forms and vocabulary which would be very hard to elicit in a more traditional approach (Hill 1990). With a bit of forethought texts can be elicited

which provide valuable insights for the learner's anthropology studies and which can also be used in discourse studies later on.

Developing her comprehension skills allows the learner to benefit from any casual exposure to the language. Those following a speech-led model practice hard and are able to produce well formed questions which tend to elicit complicated answers, beyond the competence of the learner to understand. So for example, the learner may find herself able to conduct an ethnographic interview in the language, but not understand what is being said to her. Although the comprehension-led learner may not, at least initially, frame her questions as well, she is in a much better position to understand what is being said to her, as respondents will adjust their replies in the light of her linguistic short comings and by virtue of her own comprehension skills. This is of crucial importance to those who only have a limited time available for their language learning. The comprehension-led learner is much better equipped than her speech-led colleague to carry on informal language learning once a short period of formal language learning is finished.

Drilling

At first glance the comprehension-led approach seems very attractive, not least because it appears to involve less hard work than the speech-led model; none of those tedious grammar and phonology drills. However, the comprehension-led methods actually involve just as much work as the speech-led model if not more. One distinct disadvantage of the comprehension-led approach is, as we have already noted, that there is no comprehensive guide or scheme that the learner can follow. This throws the learner very much on her own resources and requires a high degree of motivation and imagination. A comprehension-led approach requires the learner to spend long periods of time in structured listening to the language. Ideally the learner should become familiar with many hours of recordings of natural texts and must spend time with speakers of the language in nonformal settings. There is a point, too, when the learner does need to start producing the language. It is no good for her to insist that she is still not ready to speak after a few months. The comprehension-led model is no sinecure, but it is true that drills are not a very important part of the approach. Repetition of structures is important, but the focus is on the communication of a

message. Now it is true that LAMP and other speech-led methods insist that drill material should be meaningful and that the learner should think about the meaning as she works through the drills, but the stress is still on repeating the structure until it becomes a habit.

Rather than straight forward recorded drills, the learner could watch an action being repeated numerous times, while the language helper describes what is happening. There are obvious practical problems with the language helper not repeating things exactly and getting bored. Perhaps video cameras will provide a solution to these problems in the near future.

There is one area of language which is amenable to drilling, however, and that is phonology. We have already noted that although the learner's first language is not responsible for the majority of grammatical errors, there is a good deal of interference at the level of phonology or accent. Not that a foreign accent is always a bar to communication, many non-native speakers are able to communicate in a new language, while retaining a definite accent. Some writers suggest that once a learner is able to communicate adequately in a language their accent will tend to fossilize. However, it seems to me that the essentially cognitive skill of mastering the grammar of a language is very different from the psycho-motor skill involved in producing, say, a front rounded vowel. While a behaviourist model may not be adequate to describe the learner's ability to master the complexity of syntax and morphology, it may still be a good description of how the learner acquires the pronunciation of a language. It does seem that drilling of some of the more unfamiliar sounds of the new language may be a good idea.

Evaluation

Some sort of evaluation of learner progress is necessary, both to assess overall progress in language learning and to highlight areas of strength and weakness in the learner's ability. Subjective evaluation of progress by the learner herself is not a satisfactory measure and some sort of device which allows an objective evaluation is necessary. LAMP includes a self rating checklist for language ability which is very easy to use. However, the LAMP scale, as it is known, reveals its speech-led roots in that it only evaluates the learner's ability to speak the language. No attempt is made to evaluate comprehension,

reading, or writing. The LAMP scale is useful in as far as it goes, but there is a need for some way of gaining a wider evaluation of the learner's ability. In her paper on program evaluation Gloria Kindell gives examples of other self-rating scales. These cover more ground than the LAMP scale but are not as easy to use. As an appendix to this paper I have included a self-rating checklist which works along the lines of the LAMP scale but attempts to assess the learner's comprehension skills.

APPENDIX

Self-Rating Checklist for Comprehension

This self-rating checklist is based on the *LAMP scale* found in Language Acquisition Made Practical by Brewster and Brewster. However, unlike the *LAMP scale*, this checklist concentrates on the learner's ability to understand the language.

The scale is used in the same manner as the *LAMP scale*. The learner should attempt to answer all of the questions and is regarded as having reached a given level when he can confidently check all of the questions for that level. A learner on level one who can confidently check three or more (but not all) level two questions may be considered as being at level one plus. This is the same for each level.

Level One Questions

When people speak other languages around me, I can tell those who are speaking the language I am learning.

When I hear two persons speaking, I can catch a few words here and there.

I can distinguish between a question and an order.

I can understand questions about my name, my home and what I am doing in my new situation.

When someone is introduced to me, I understand his name and where he comes from.

If someone describes how to find a building in my neighborhood, I can follow their directions.

Level Two Questions

If someone describes an event, I know on which day and at what time of day it happened.

If someone asks me about one of my neighbors, I can understand to whom they are referring.

In a casual exchange of greetings I can understand everything said to me.

When eating with friends, I can understand comments about the food if they are addressed directly to me.

If someone describes the village to me, I will understand where to find the church, the market, or other prominent features.

Level Three Questions

If I call to see someone and they are not there, I can understand what I am being told about my friend's absence.

As long as my coworker is willing to repeat once in a while, I can understand when he tells me how he spent the previous day.

I can understand several short sentences in a row during a normal conversation.

If someone describes to me about how to do a simple task, I will be able to follow their directions.

If I hear a sermon based on a passage of the Bible I am familiar with, I can follow the main idea.

I can follow a description of some facet of local culture which interests me.

Level Four Questions

When people are speaking together about local problems I can understand enough to form an opinion about the matter.

I can understand snatches of conversation which I happen to overhear.

I can understand what is being said when someone is shouting some distance away.

I understand the rules for turntaking in conversation and can recognize different types of discourse from the way the language

is used. I know when someone is speaking in an inappropriate manner.

I can understand humor and language puns.

Level Five Questions

When people speak to me in the new language, I reply without really being conscious of which language is being used in the conversation.

I am sufficiently fluent to be able to understand proverbs and stories.

I understand discussions in the language as well as those in my mother tongue.

I can follow descriptions of quantity which involve large numbers without needing to translate them into my mother tongue in my head.

Listening to the new language is no more exhausting mentally than listening to my mother tongue.

People make no effort to speak slowly or explain cultural details when they are addressing me.

Notes

[1] Some authors, notably Krashen, draw a distinction between language learning, which is seen as the conscious process of learning grammatical rules and structures, and language acquisition seen as an unconscious, automatic process. No such distinction is intended in this paper and the terms are used interchangeably.

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Report on the Australian Linguistic Institute

Robert Early

SIL Vanuatu and The Australian National University

Many of us from further parts (deictic centre = North America) have been aware, enviously, of the Institutes run by the Linguistic Society of America for many years. Now we can boast our own South Pacific version, with the successful completion of the Inaugural Australian Linguistic Institute, which was held at the University of Sydney, Australia, for the two weeks from the 29th June until the 10th July.

The Institute was sponsored by the Australian Linguistics Society and the Applied Linguistics Association of Australia, with the heavy burden of conceptualization, promotion and administration being carried by the staff and students of the Linguistics Department, University of Sydney, under the chairmanship of Professor Bill Foley. The 300 or so registrations greatly exceeded organizers' expectations, and while there was just a handful of international participants, there were pleasing representations of teachers of linguistics, researchers, and post-graduate students from many different institutions throughout Australia and New Zealand.

The Institute provided courses and workshops on many topics, some of which could be taken for credit from different universities. There were more people offering to teach courses than could be fitted into the time available, so the organizers were able to present a selection of good quality courses. The most well-received were those that had an important international teacher as a drawcard. These included a course on non-linear phonology by Nick (G.N.) Clements (Institute de Phonetique, Paris); one on LFG by Joan Bresnan (UCLA); and a workshop on Space in Language and Interaction in Aboriginal Australia by Stephen Levinson (Max-Planck Cognitive Anthropology Research Group). Other course offerings included: Aboriginal English, Pidgins, and Creoles; conversation analysis; language and gender; language and race; cross-cultural pragmatics; various courses in applied linguistics topics such as language acquisition, planning

and testing; the languages of the Pacific; topics in Australian languages such as linguistic prehistory and semantic change; systemics; and studies on Australian and New Zealand English. There was also a course on translation, which focused mainly on methods and curricula for training interpreters and translators in the context of multicultural Australia, but included a session on translation in PNG by Graham Scott (La Trobe University, chairman of the SPSIL School committee), and one on translation in aboriginal Australia by Chris Kilham (AAIB). Chester and Lynn Street, along with Chris Kilham and Margaret Mickan (all from AAIB), were also responsible for running a training course in translation for a small group of aboriginal participants. Other SIL members at the Institute were Jenny Lee (AAIB), Crispin Lee (SPSIL), and Robert Early (Vanuatu/ANU).

There were several other major linguistic events held in conjunction with the Institute. Before the Institute, one could attend the one or two day annual conferences of the Australian Linguistic Society, and the Australasian Lexicography Association. During the Institute, the Australian Celtic Studies Conference sessions were open to Institute participants. After the Institute, the Applied Linguistics Association of Australia held its annual conference for several days, and in the week following one could attend either the International Systemics Functional Congress, or the Pacific Second Language Research Forum, an inaugural forum on second language acquisition research. Billed as the "Winter Linguistics", this three-week series provided adequate variety for the most discerning palate, and ample measure for the most enthusiastic appetite. Most people attended just a selection of these events, but for all, the Institute was the highlight.

The Institute had early bird registration fees of $120 (U.S.) for students and $200 for others, and on-site accommodation was available for $35 per night. Most people felt that they were getting good value for their money. It looks as if the Australian Linguistic Institute will become a regular biannual feature, with the next one planned for July 1994 in Melbourne. This would be a good opportunity for SIL members on study programs or on furlough, or consultants from nearby entities, to update in various areas of linguistics. ■

The Second International Conference on Maintenance and Loss of Minority Languages

Report by Ken Decker
South Asia Group

The Second International Conference on Maintenance and Loss of Minority Languages was held September 1 through 4, 1992 in Noordwijkerhout, The Netherlands. The conference was attended by about 125 people representing over thirty countries. The Summer Institute of Linguistics was well represented by seven members: Ken Decker (South Asia Group), Tom Hemingway and Merieta Johnson (Central America Branch), Steve Quakenbush (Philippines Branch), Clinton Robinson (Cameroon Branch), and Mary Morgan and Roland Walker (International Programs).

There were forty-four speakers presenting papers and a poster session including about ten more presentations. About eighty percent of the topics were concerned with immigrant communities, or speakers of major world languages living as a minority community in a western country dominated by another language, i.e. French speakers in Canada, Vietnamese in Finland, English in Norway, Turkish in The Netherlands, etc. The other twenty percent of the papers were concerned with indigenous groups which are minorities in their own countries, i.e. Berber in Morocco, Kalasha and Phalura in Pakistan, Ainu in Japan, K'ich in Guatemala, etc. Some of the presentations were of a more theoretical nature while the majority focused on the present issues concerning minority ethnolinguistic groups struggling with language issues. The topics covered the entire range of sociolinguistic concerns, including: childhood bilingualism, second language proficiency, linguistic vitality, language maintenance and loss, political change and language policy, and ethnolinguistic identity.

The prominent message of the conference was to uphold the intrinsic value of minority groups and their languages, and the benefits of linguistic variety in society. Joshua Fishman, in his concluding

remarks on the conference said, 'We value ethnolinguistic democracy for greater dignity to be given to all'. He referred to the violence and 'ethnic cleansing' in former Yugoslavia as an example of the seriousness of the maintenance and loss of ethnolinguistic minorities. In a general reference to the rapid loss of minority languages around the world, Fishman said that 'the avalanche of language shift is like a cancer on the skin of humanity.' However, he questioned if our western views of ethnolinguistic democracy and equality may be another form of oppression of the peoples of the developing countries; they may not want ethnolinguistic equality.

Professor Fishman recommended that we would benefit from hearing more from members of minority language groups—activists who are promoting their languages. He noted that these people were obviously absent from the conference and several had canceled, presumably because of the prohibitive cost of attending. Fishman concluded with a plea for all present to speak out for the minority language communities, to give them a voice, to help the world hear of them or they would not be heard. He said, 'We need to respond with the intent to have impact.'

It is my observation that we in SIL are in a unique position, on a worldwide basis to both speak out for the minority language communities Professor Fishman was referring to, and to also help the people of those groups voice their concerns to a wider audience. I believe that we in SIL share, with these language specialists, a respect for the right of minority language groups to exist and the right of self-determination.

Personally, it was very rewarding to have the opportunity to attend an international, professional conference and to present a paper. The experience was useful and I was able to gain useful information for my master's thesis, which I am completing at this time. It was good to have contact with other linguists and language specialists from outside of SIL and from around the world. I wish to thank SIL International Administration, Academic Affairs, and South Asia Group for the financial assistance which enabled me to attend this conference. ■

Second Encounter for Linguistics in Northwestern Mexico

Reported by Thomas Willett
Mexico Branch

The II Encuentro de Lingüística En El Noroeste (Second Encounter for Linguistics in Northwestern Mexico) took place in Hermosillo, Sonora, Mexico from November 18-20, 1992. It was jointly organized by the Department of Letters and Linguistics at the University of Sonora and the Regional Center of the National Institute of Anthropology and History, and was co-sponsored by several other Mexican institutions and the Universities of Arizona and Utah.

The Encounter was attended by about one hundred linguists and students from twenty-two institutions: sixteen Mexican, five American, and one European. Half of the fifty-plus papers presented were ethnolinguistic analyses of indigenous languages of Mexico or western U.S. The others treated topics ranging from the analysis of Spanish to language learning and teaching. An interdisciplinary session treated the overlap between linguistics and medicine, literature, history, and sociology, and an invited speaker sketched the origins and current trends in semantics.

This conference provided an excellent opportunity for SIL members working on languages in the region to interact with many well-known Mexican and American linguists. Six of us attended and gave papers. Not only was our kind of language description well received, but we were also invited to help as guest lecturers and advisors to some Mexican institutions offering degrees in linguistics.

During the conference, an agreement was reached to form a Linguistic Association of Northwestern Mexico, which will be formally constituted at the third Encuentro in November of 1994. The smoothly-run sessions, held in a prison-turned-museum, were complemented by two traditional Mexican dinners and a narrated demonstration of local ethnic folk dances. These events added to the

congenial atmosphere of the conference which, along with the high level of scholarship, was repeatedly applauded by the participants. ■

29th Regional Meeting of the Chicago Linguistic Society

April 22-24, 1993

GENERAL SESSION – APRIL 22-23

Invited Speaker
Hans Henrich Hock,
University of Illinois at Urbana-Champaign

PARASESSION – APRIL 23-24

What We Think, What We Mean, and How We Say It:
The Role of Conceptual Representation in Language

Original unpublished work on the relationship between conceptual (and/or semantic) representations and grammar are welcome.

Invited Speakers

George Lakoff, University of California, Berkley
David Dowty, Ohio State University
Michael Silverstein and Lawrence Barsalou, University of Chicago
Lila Gleitman, University of Pennsylvania

Deadline for receipt of abstracts is Feb. 12, 1993.

Send abstracts to:
Chicago Linguistic Society;
1010 E. 59th Street, Chicago, IL 60637

For more information, or to get on e-mail list:
cls@sapir.uchicago.edu
(312) 702-8529

Conference On the Relationship Between Linguistic and Conceptual Representation

David Tuggy

The 1992 meeting of the Belgian Linguistics Society in conjunction with the International Pragmatics Association was held in Antwerp from the 26th through the 28th of November. The theme for the conference was 'The relationship between linguistic and conceptual representation'. About 70 presentations were scheduled, some 19 of them being 'poster' presentations. The following are the invited speakers and the titles of their presentations:

> Herbert H. Clark (Stanford) on "Means of Meaning".
>
> Annette Herskovits (Wellesley) on "'Across', 'along', and other ways to travel: exploring the interplay of linguistic knowledge and perceptual geometry".
>
> Willem J. M. Levelt (Max-Planck-Institut für Psycholinguistik) on "The language of space, perspective taking and ellipsis".
>
> Dan I. Slobin (UC Berkeley) on "Two ways to travel: Verbs of motion in English and Spanish".
>
> Leonard Talmy (SUNY-Buffalo) on "The windowing of attention in language".

Besides their presentations (in plenary sessions) there was a final round table discussion among the invited speakers, chaired by Steven Levinson, from the Cognitive Anthropology Research Group at the Max-Planck-Institut.

Several themes stood out to me.

(1) There seems to be a growing consensus that semantics is cognitive in nature. This, of course, is something Grandma could have told us, but evidences that it is taken very seriously abound. Among its implications is a general rejection of a strictly 'modular' approach in which semantics is hermetically sealed off from the rest of cognition and permitted to operate in ways unrelated to how the rest of cognition functions. Many 'grammatical' phenomena (e.g. subjecthood, semantic 'gaps' in syntactic structures, textual coherence

devices) are being related to clearly cognitive phenomena such as attention, perspective, foregrounding and backgrounding, figure/ground organization, etc. Russell Tomlin (U Oregon), for instance, reported experiments in which subjects described scenes in which their attention had been prompted to rest on one of two otherwise perceptually equal figures, e.g. in a video a red fish and a blue fish swim towards each other. Just before they meet, subjects' attention is drawn to the blue fish. At meeting, one fish eats the other. If the red fish eats the blue fish, 97 percent of subjects will describe the situation with a passive; if the blue fish eats the red, they will use an active. Conclusion: manipulation of attention determined choice of grammatical subject: subjecthood must be closely linked (if not at some level identical) to focus of attention.

(2) The climate seems to be warming towards some sort of neo-Whorfian view of the language-mind relationship. No one wants to claim that human thought is in any strong sense *determined* by the language/culture complex, but many are willing to believe that it is heavily *influenced* by it. Again, Grandma could have told us this, but some are working vigorously to get hard evidence for it. A fascinating study at Levinson's group at the Max-Planck-Institut involved Guugu-Yimidhirr (G-Y) speakers from Australia, who locate objects linguistically in terms of cardinal directions rather than by reference to canonical human orientation (e.g. by saying 'the man to the south of John' rather then 'the man to John's right'). These people will locate north within three degrees or so from each other, whereas Europeans' locations of north, when they can locate it at all, vary as much as ninety degrees. Further, if G-Y speakers are shown a configuration of items while seated at a table facing north and then asked to recreate it on another table while facing south, they consistently reproduce the cardinality of the configuration, e.g. putting a toy man facing south and standing to the west of a toy house. This is just backwards from the Europeans, who instead preserve orientation relative to the viewer, e.g. putting the man consistently facing the viewer and to the (viewer's) left of the house. It seems pretty clear evidence that G-Y speakers *think* about location differently from Europeans.

(3) Granted that language/culture may influence cognition, it is important that data be adduced, especially data correlating linguistic

with non-linguistic cognitive patterning, from many different cultures and languages.

(4) Granted that people in different language/cultures may *think* differently from each other, translation may not be as easy as some have theorized it should be. For instance, Slobin's paper showed that English speakers and writers of all ages typically use expressions with a verb indicating manner of motion, followed by a complex path description (e.g. 'He loped down the trail, along the ridge, and up over the rocks to the foot of the cliff'). Spanish speakers do not use such expressions, and they can only be awkwardly translated into Spanish. The best translators typically leave them out or shorten them dramatically (e.g. 'He ran to the foot of the cliff'). The reasoning apparently is that to include all the information (which Spanish speakers are evidently not all that interested in anyway) would muddy the passage more than the information itself is worth. In effect, you do not naturally say quite the same thing in the two languages, and if you try to preserve all the information in a translation, it will be at the cost of naturalness.

This was, to my mind, a very worthwhile conference to have attended, and it was worthwhile to have had SIL represented at it.

Besides attending the conference and presenting a paper there, I was able to interact with linguists at the Max-Planck-Institut in Nijmegen, Netherlands, and more extensively at the Katholieke Universiteit in Leuven, Belgium. I gave a lecture at each of these places, and it seemed to be well received. ■

REVIEWS OF BOOKS

Studies in the Linguistic Sciences 18:1.
Edited by Michael J. Kenstowicz. Urbana, IL: Department of Linguistics, University of Illinois, 1988. 177 pp. $6.00.

Reviewed by John M. Clifton
Papua New Guinea

Studies in the Linguistic Sciences is a journal published by the Linguistics Department at the University of Illinois. This issue contains seven articles: five dealing primarily with phonology, one with morpho-syntax, and one with dictionaries. Areally, three deal with sub-Saharan African languages, one with Arabic, two with Spanish, and one with Lakhota.

Two of the articles dealing with phonology present autosegmental analyses of tone in African languages. In *Tonology of noun-modifier phrases in Jita*, Laura J. Downing treats some of the tone processes in Jita, a Bantu language. A noun may be toneless, or it may have one high tone which is linked to a given syllable. A high tone generally shifts one syllable rightward when phrase medial, even across word boundaries. There is a group of nouns in which the high tone always surfaces on the penultimate syllable, it never shifts. Downing uses the apparent nonapplication of the shift to argue that these roots have high tone on the antepenultimate syllable, not the penultimate syllable. Thus, an apparent anomaly is shown to follow from an underlying difference.

Within noun-modifier phrases, Downing argues a high tone is associated with the final syllable of any word preceding a modifier phrase. This results in additional high tones in certain noun-modifier phrases. A particularly important aspect of this analysis deals with the proper syntactic domain of the rule. As such, it provides a good example of how syntactic factors have been used to constrain phonological rules.

In *Tonal polarity in two Gur languages*, Michael Kenstowicz, Emmanuel Nikiema and Meterwa Ourso (KNO) examine apparent

polar tones in the West African languages Moore and Lama. In Moore nominal number suffixes frequently take a tone opposite from that of the root: *kòr-gó* 'sack' versus *wób-gò* 'elephant'. KNO propose that the underlying representations of these suffixes have high tone which becomes low after a high tone root. Thus, the apparent polarity is a surface reflection of a dissimilation process. This analysis also accounts for the many forms like *míu-gú* 'red' in which the root and suffix both take high tone. These roots are postulated to be underlyingly toneless. When suffixed, the high tone from the suffix associates with the root, and then spreads to the suffix. Similar processes are shown to occur in Lama, although the tonal system is more complicated. Crucial reference is made in underlying representations in both languages, then, to a three-way distinction between roots with high ([+high]) tone, low ([-high]) tone, and no tone, contrary to the claims of Underspecification Theory (cf. Archangeli and Pulleyblank 1989). Both languages are also shown to have a grammatical particle in associative constructions consisting entirely of a low tone, although it manifests itself differently in the two languages.

In another paper dealing primarily with phonology, *Some morphological and phonological interactions in Lakhota*, Trudi A. Patterson discusses Ablaut and Velar Palatalization in Lakhota, a Siouan language from North America. Patterson argues that these processes can be best handled within the theory of Lexical Phonology, if it is suitably weakened and cyclic application is still allowed. I find it troubling that strata are posited solely on the basis of the order of morphemes and the domain of phonological rules. With no semantic/syntactic justification for the domains the analysis seems ad hoc.

In one of two papers dealing with Spanish phonology, Maria Carreira discusses *The representation of diphthongs in Spanish*. On the basis of stress assignment, Carreira argues that while falling diphthongs (e.g. [aw]) must originate in a single syllable, rising diphthongs (e.g. [we]) must originate as sequences of vowels. In this analysis the difference in syllabification between *Máryo* and *Marío* is a consequence of the difference in stress assignment. This results in allomorphy in pairs like *manía* 'obsession' versus *manyático* 'maniac'. Carreira also proposes that potential counter-examples like [boío] *bohío* 'hut' in which the [i] is a separate rhyme instead of an onset (as in [oja] *oya*

'pot') can be accounted for by positing empty skeletal slots in the underlying forms.

There are a number of typographical errors in the article which make it difficult to follow in places. The representation of the OR (onset-rhyme) structure is incorrect in a number of examples. For example, in (8)

/s k a t i u/ should be /s k a t i u /.
O R O R R ORORR

The erratic indication of stress placement also leads to incorrect claims such as *[en.fer.mo], instead of *[én.fer.mo]. In spite of these errors, the article should be of interest to anyone struggling with the common problem of whether glides (semivowels) are underlyingly consonants or are derived from vowels. It illustrates nicely potential interaction of stress and glides.

In the final phonological paper, *On deriving specifiers in Spanish: morpho-phono-syntax interactions*, Uthaiwan Wong-opasi discusses various occurrences of *el* in Spanish. Wong-opasi claims that in constructions like *el água* 'the water' *el* is not a masculine article used before a feminine noun. Instead, it is the realization of the feminine article *la* as a result of truncation of *a* and epenthesis of *e*. Justifying this analysis within the broader framework of specifiers in general, Wong-opasi argues that the required phonological rules must make reference to morphological and syntactic factors.

While Wong-opasi argues that the single form *el* has more than one source in Spanish, in *A functional typology of 'ni' in Kivunjo (Chaga)* Lioba Moshi argues that the apparently polysemous form *ni* has only one basic meaning. Moshi examines the syntactic distribution and the phonological effects of the various occurrences of *ni* and concludes *ni* always marks focus. The pragmatic considerations in the use of *ni* are not discussed in this article. In addition, the semantics of focus are asserted, not justified. However, the morpho-syntactic argumentation in the paper is presented well.

The remaining article, *Exploring the dictionary: on teaching foreign learners of Arabic to use the Arabic-English dictionary*, by Omar Irshied and Peter Whelan, deals with a problem in applied rather than theoretical linguistics. The problem addressed is how to teach

second-language speakers of Arabic to use a dictionary—no easy task. All Arabic verb roots have up to ten 'derived forms', each of which has a derived active and passive participle, all of which are listed only under the root. For example, *aktaba*, *takaataba*, and *mukaatab* are all listed under k-t-b. The exercises suggested by Irshied and Whelan may be useful for those developing dictionaries in languages in which a number of stems are created from a single root by prefixation.

Overall, the articles in this volume represent well-argued analyses of phenomena in a number of diverse languages. Kenstowicz is probably one of the most lucid writers in linguistics, and the article by Kenstowicz, Nikiema and Ourso is a particularly good article for anyone who wants an example of solid linguistic argumentation.

Studies in The Linguistic Sciences is produced in the tradition of conference proceedings from the Chicago Linguistic Society, Berkeley Linguistic Society, and so on. Contributors submit their articles in camera-ready form, so each article appears straight from the author's word processor. The disadvantage of this format is that there is less quality-control over typographical errors, and there is indeed disparity between articles in this regard. At the same time this helps keep down the cost of the journal, and there is less of a time lag between when the articles are written and when they appear. As a consequence, the articles represent current linguistic theory.

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Cambridge studies in linguistics No. 61.
Universal grammar and language learnability. By Anjum P. Saleemi. Cambridge: Cambridge University Press, 1992. Pp. 168. $44.95.

Reviewed by David J. Holbrook

Anjum Saleemi's *Universal Grammar and Language Learnability* merges Chomsky's theory of universal grammar with the formal approach to learning (i.e. learnability theory). He argues that any theory of language acquisition must be linked with a theory of learnability if it is to be considered a cognitive system. Saleemi provides a 'discussion of the basic puzzles of language learning', and in the process does 'an analytic review of certain key aspects of the linguistic and learning theories' (p. 1). He gives a thorough analysis of the parameter-fixation model of language acquisition and puts it in the context of a number of issues related to learnability theory. Specifically, parameter-fixation is conceptualized as an explicit model of domain-specific learning (i.e. the specific domain of language in this context).

Anjum Saleemi received his Ph.D. from the University of Essex. This book has evolved from his doctoral dissertation and is written in a highly theoretical framework. Dr. Saleemi is a professor at the Allama Iqbal Open University in Pakistan. The content of his book is written in a technical style and is aimed at those who have a strong background in language acquisition and learnability theories, especially Chomsky's theory of universal grammar.

The book is organized into eight chapters, the first of which provides an overview of the contents of the book and lays the groundwork for the merging of universal grammar with learnability theory in an expanded theory of language acquisition. Chapter two provides an analysis of universal grammar and its relation to learnability theory. Chapter three lays the foundation for a systematic theory of language acquisition within the framework of a learning paradigm. A learning paradigm consists of explicit characteristics of what is learned, how it is learned, who learns it, and under what conditions it is learned. Chapters four and five are concerned with a parameter-setting model of language learnability. Chapter four introduces two approaches to a parameter-setting (parameter-fixation) model: the formal approach

and the developmental approach. Chapter five provides a critical analysis of these two approaches. The sixth chapter reanalyzes the licensing and parametric mechanisms underlying the null-subject parameter. Saleemi proposes a null-subject parameter that is multi-valued where others have previously posited a binary-valued null-subject parameter.

Chapter seven discusses the idea of augmented parameter-fixation (i.e. the process of setting parameter values coupled with observational learning). Parameters are said to generate languages. Using the null-subject parameter, the author demonstrates that the use of null-subjects in languages requires a form of observational learning. The null-subject parameter merely sets the boundaries within which the null-subject can be realized. The specific uses of the null-subject within those boundaries are learned observationally in each specific language.

Chapter eight provides a good summary of the material, as well as proposes some problems for further research.

The book is well written and organized, but, as mentioned earlier, is of a highly theoretical nature. Saleemi's writing style is quite technical, and the material covered may be too advanced for an ordinary working linguist. Those, however, interested in Chomsky's universal grammar coupled with various theories of language acquisition and language learnability, will find the book to be of deep interest. ■

Language Change: Progress or Decay?
By Jean Aitchison. New York: Cambridge University Press, 1991.
2nd Edition. Pp. 221. Hardback $49.50. Paperback $14.95.

Reviewed by Michelle M. Olson.

Language Change: Progress Or Decay? is a helpful book that can raise the awareness and sensitivity of beginner linguists and literacy workers to the complex issues of language change, which, if the new

awareness is applied, could enhance their work. The book should be of interest to seasoned linguists because of its insightful integration of many scholastic disciplines, and of interest to the general public because Aitchison shows how we all participate in language change every day by our conscious and unconscious choices of speech.

Jean Aitchison is Senior Lecturer in linguistics at the London School of Economics. She has authored several books and articles on linguistics, her main interests being historical linguistics and psycholinguistics. She has traveled and lectured widely in India, Nepal, Papua New Guinea, USA, and Europe.

The purpose of the book is to show that language change is neither progress nor decay. Language change happens. Aitchison details how it happens, showing the fascinating interplay of the diffusion of elements of language, phonological processes, sociolinguistics, and the human need to maintain clarity of grammatical constructions. She also discusses how languages are born, and how they sometimes die. Aitchison keeps the emotional aspect of language change to the forefront as she discusses all this material.

Nothing of what Aitchison says about language change comes as a surprise. Based on theories of change of other experts (Kuhn, Rodgers, Wallace, Whiteman, to name a few), one would expect language change to occur exactly as Aitchison describes, including the sharing of elements of language, resistance to and promotion of the changes, and the fact that if language change were shown on a graph it would look like an S-curve with a slow-fast-fast-slow rate of acceptance. Aitchison's book is unique in that she uses language as the example that illustrates already well-known models of change. Since language permeates every aspect of human life, language change is an emotionally loaded issue, probably of interest and significance to anyone who would take the time to think about it.

The book is divided into four parts, each part having three to four chapters. Part 1 introduces change as a normal, continuous human process, and also discusses how linguists study language change. Part 2 describes in detail exactly *what* occurs when a language, or an element of a language, changes. Part 3 describes *why* languages change in terms of sociolinguistics, phonological processes, grammar, and keeping order and symmetry in a language. Part 4 discusses

some theories as to *how* languages change, and the birth and death of languages. Aitchison discusses pidgins as potential beginnings of new languages and discusses the processes of decreolization and replacement as two ways that languages may die. The final chapter is the summary and conclusion of the book—*Progress or decay? Assessing the situation*. The book also has some helpful resource sections, including a table of linguistic symbols and terms, sections of references, suggestions for further reading, and a good index.

It is doubtful whether linguists before our time were as incompetent as Aitchison often implied (pp. 36, 37, 76-77). I would have felt more positive toward the book if she had credited the earlier scholars with helping to direct her own work and if she had recognized that the earlier linguists themselves may have been in a process of resisting new changes in linguistic theory (Kuhn, 1970). Instead, her comments toward earlier linguists clashed against the content of the book.

Also, there was one small comment which needed documentation: Aitchison claims that the point at which people notice and object to language changes is 'arbitrary' (p. 128). I doubt it, and I'd like to see some data. Aitchison's claim could be upheld if the reader could see these arbitrary points of complaint plotted along the S-curves of change of languages which have been studied. If there is truly an equal distribution of the points at which complaints against language changes occur, and no changes in their intensity and frequency, then the arbitrary nature of these complaints may be an interesting anomaly, according to other models of change.

An example of Aitchison's edited style is on p. 44 where she uses 'who' instead of 'whom'. I had to stop reading to try to understand who did what to whom. I wondered if she was trying to make a case in point, in addition to the thesis of her book (pp. 10-11). However, it was this irritation that kept me reading and helped me to realize that language change is indeed as emotionally volatile an issue as she paints it! (See p. 169. Informal grammar is becoming more standard. I went on to review the book because I wanted to own it!)

Aitchison's informal, lightly edited style seemed incongruent with her high level of education. Despite this minor irritation, reading the book was well worth my time as a beginner linguist and translator. The book has given me some new insights into language change that

will enhance my work. In addition to its helpfulness, the book is simply good reading. The entire book is insightful, informative, and is written in a straightforward, lively style. I highly recommend the book.

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References and some interesting models of change that agree with Aitchison's description of language change (and which may disagree with some of her remarks):

Kuhn, Thomas S. 1970. The structure of scientific revolution. Second edition. Chicago: University of Chicago Press.

Rodgers, Everett M. 1962. Diffusion of innovations. Third edition 1983. New York: The Free Press. (Includes how change occurs—with an S-curve.)

Wallace, Anthony F. C. 1956. Revitalization movements. American Anthropologist. 58.264–281. (Describes stages of change, including unrest, disequilibrium, and finally a new steady state.)

Whiteman, Darrell L. 1983. Melanesians and missionaries. An Ethnohistorical Study of Social and Religious Change in the Southwest Pacific. Pasadena: William Carey Library. (Combines several models of change—to produce an eclectic model, p. 8.) ■

Infinitival complement clauses in English:
A study of syntax in discourse.
By Christian Mair. Cambridge University Press, 1990.
Pp. 264. $49.50

Reviewed by Richard L. Watson

The first eighteen pages of this volume introduce the scope and method of the study, which encompass not only a comprehensive study of infinitival clauses in English, but one done in such a way as to provide evidences for the importance of being corpus-based and being 'functional', as opposed to 'formal'. In the words of the author:

> It is to be hoped that a corpus-based analysis which observes the five guiding principles outlined above can be a first step towards a 'functional' approach to English complement clauses worthy of the name—that is an approach which systematically relates syntactic facts, structural or distributional, to nonsyntactic phenomena, such as, for

> example, the conventions of human communication or the limitations of human perception or cognition. (p. 10).

The five guiding principles referred to are:

1. Focus on the British English standard;
2. Inclusion of the spoken language on an equal footing with edited written language;
3. Focus on the fuzzy edges of syntactic structures and categories;
4. Focus on the stylistic and statistical dimensions of syntax;
5. Focus on the intersection of syntactic and discourse structure.

Mair also states:

> Insofar as the present investigation is devoted to 'the systematic and quantified study of syntax in discourse' (Givón 1984:11), it is clear that only a corpus can be the database (p. 6).

The corpus used, the Survey of English Usage, founded by Randolph Quirk, is impressive. It contains 179 written and spoken texts totalling 895,000 words, with all the advantages of computerized systematization. However, Mair assures us that even that is not meant to be restrictive—other data is also used, with appropriate safeguards, when relevant.

Mair is not only bent upon exemplifying a 'functional' description 'worthy of the name', but also provides a keen argument against the belief that the syntactic structure of a language is 'a well-defined autonomous formalism'. The author particularly attacks Newmeyer (1983:10, 111) for imputing *to the functionalist the patently absurd belief that there is a 'direct' or 'one-to-one' match between syntactic form and communicative function...* Along with many other keen challenges and frequent support from Talmy Givón and others, Mair points out:

> Newmeyer himself occasionally acknowledges that the evidence points either way (e.g. p. 24), so that it seems premature to decide the question whether the relevant portions of the human brain are the seat of an innate grammatical-syntactic faculty or a more integrated cognitive and communicative faculty.

This side of the polemic is especially interesting when read alongside of Dan Everett's 'Formal Linguistics and Field Work' (NOL 57:11-26).

I agree with Dan's thesis that SIL field-workers need good training in syntax, but I think that he, like Newmeyer, overstates both the importance of 'formal' syntax and the supposed failure of functionalists to be concerned for representational as well as communicative function.

Chapter 2 deals with 'Infinitival subject clauses and some semantically related types of infinitival complementation'. Chapter 3 deals with '*to*-infinitival clauses as complements of transitive verbs'. Chapter 4 provides a summary and conclusion. There are also three appendices, extensive notes, references, and an index.

Regarding the restriction of the study to infinitival clauses, Mair states:

> ...the *to*-infinitival clause is certainly not a bad structure to focus on. Not only has it been the most frequent type of nonfinite complement clause throughout the history of English... it is also extremely flexible functionally, so that a thorough study of infinitival clauses will not fail to shed light on the various textual and communicative functions performed by sentential complementation in modern English (p. 2).

As we get into chapter two, the complexity, as well as flexibility become evident. Infinitival subject clauses are divided into four types, dependent upon presence or absence of a subject (a *for* phrase) and of extraposition (moved to the end of the matrix clause with *it* functioning as a dummy subject). (I have taken the liberty to simplify the examples and omit their references in the corpus.)

a) extraposed and subjectless: (650 examples in the corpus)
1) ...and it's frustrating, ...beyond belief, *to be so close to these important centres*...

b) extraposed, with subject: (100 examples)
2) However, after the noble earl's death...it proved possible *for the curve to be straightened out*.

c) non-extraposed and subjectless: (52 examples)
3) *To disregard the past* is very often the surest means of becoming its slave.

d) non-extraposed, with subject: (5 examples)
4) *For ratepayers to have to look on helplessly as their money is used to prolong strike action*..., is galling and offensive. ('with subject' are also referred to as '*for*-infinitival clauses')

The 'subjectless extraposed' clauses are the best known, so Mair only summarizes the findings concerning them and is more interested in investigating:

a) the factors preventing infinitival subject clauses from occupying the statistically normal extraposed position,

b) the function of *for*-infinitival subject clauses within the system of sentential complementation in English (p. 22).

The one verb which occurs most frequently in the matrix predicate (also called 'superordinate predicate'), is *take*. In general, verbal predicates involve 'cost', as in 'it takes money to…', or an emotional or cognitive reaction to an action, as in 'it pleases me to…' However, predicates embedding infinitival subject clauses are typically adjectival and nominal. These are divided into five semantic categories: those expressing potential, degree of ease or difficulty, frequency, judgment, or degrees of necessity or desirability. For example: 'it's possible to…'; 'easy to…'; 'not uncommon to…'; 'stupid to…'; 'good to…'

When the predicate is filled by a verb, it is rare for the subject position to be filled by an infinitival clause because verbs tend to call for more noun-like subjects. Dynamic predicates particularly call for animate or human agents, accounting for the greater frequency of nouns, followed by 'that' clauses (incl. 'the fact that'), then gerunds, then 'it' with an extraposed infinitival clause. A general rule of speaker behavior could be stated as: 'Avoid using a clausal subject, but if you do, make it a gerund or else try to extrapose it' (p. 27).

Under the theme of 'corpus-based' analysis, Mair points out the discrepancy between the notion in autonomous syntax of prototypical structures versus statistically validated structures. For example, TG posits the following as the prototypical structure for all infinitival subject clauses:

[for + NP + to-inf. cl.] + matrix predicate;

contrary to the fact that the extraposed subjectless kind is the statistical prototype, i.e.,

it + copular verb (mostly be) + adj.phr. + to-inf.cl (p. 29).

The alleged structural prototype rarely occurs in speech or writing. The statistical prototype, subjectless extraposed, is structurally the furthest removed. Reasons for the statistical prevalence are that subjectless nonfinite clauses are a useful means of syntactic compression because they free the speaker or writer from the need to mention explicitly what can mostly be inferred from the context, and the extraposition of clausal constituents is in line with the principle of end-weight in the information structure of an English sentence. Mair prefers the term 'intraposed', presumably because the infinitival subject can as well be considered to be 'left-dislocated' when it occurs at the beginning of the matrix clause and, even at the 'end', it can be followed by other matrix clause information, e.g. 'It is dangerous *to go there* for all but the most experienced skiers'.

> At a more fundamental level, one might question whether the tracing back of extraposed and reduced clauses to more complex and regular underlying patterns is a useful method of grammatical analysis, especially if it involves deriving perfectly natural structures through intermediate stages which are either extremely rare or downright unacceptable (pp. 30-31).

Having described the statistically most common type of infinitival subject clause, Mair asks the question,

> If there is a natural tendency for subject clauses to be extraposed, what, then, are the conditions in which there is no extraposition, or—to use functionally motivated terminology—what are the conditions which favour the intraposition of clausal subjects?

Mair then describes three factors: syntax (largely the converse of reasons for subjectless extraposed above), information packaging and style. The author's conclusion regarding information packaging involves the fronting of marked themes and is stated as follows:

> ...absence of extraposition always implies that the speaker or writer wishes to achieve one or both of the two following effects.
>
> a) appropriate distribution of given and new information *within a single complex sentence*...(p. 37); [or]
>
> (b) strengthening of cohesion *across sentences*: Infinitival subject clauses which contain anaphoric references to previous sentences are less likely to be extraposed than others because the non-extraposed clause serves as a cohesive device...(p. 38).

Regarding stylistic factors, Mair states that they give a formal flavor:

> First, a non-extraposed clausal subject is a gross violation of the 'light-subject constraint' which seems to apply very rigorously in unplanned speech... Secondly, the cohesive functions performed by non-extraposed infinitival subject clauses in written English are taken up by alternative strategies of topicalisation in the spoken language (p. 39).

Regarding further strategies of topicalisation, Mair is using terms widely current in the literature on sentential complementation in English, but

> ...that terms such as 'movement' and 'raising' should not be used except as metaphors loosely capturing the reordering of chunks of information in semantically related patterns of infinitival complementation, and that any attempt to connect 'moved' or 'raised' forms with their supposed underlying forms through autonomous syntactic mechanisms is bound to fail (p. 55).

In this regard Mair presents the following four sentences:

(96a) It is not easy to answer the question.
(96b) To answer the question is not easy.
(96c) The question is not easy to answer. (Tough-Movement)
(96d) It's not an easy question to answer. (Pseudo-tough) (p. 57).

The author notes that early TG analysts called (96c) 'Tough-Movement' because they tried to derive it from (96a), and calling (96d) 'Pseudo-Tough Movement' because it has the same characteristic gap in its infinitival complement clause. However, no concern is shown with hypothetical derivations or deletions, but rather that the four present a paradigm of stylistic options and the questions which this raises.

Syntactically Mair shows that:

> Tough-Movement structures are not only different from corresponding sentences with infinitival subject clauses in the order of constituents. There is also a potential difference in meaning because Tough-Movement structures provide the take-off point for an additional SVC interpretation which is not available for complex sentences with infinitival subject clauses (p. 65).

Functionally the conclusion is reached that Tough-Movement is a 'topic-creating mechanism, strengthening ties of syntactic cohesion

across sentences'. Secondly, it 'may lead to structural simplification *within sentences*...' And thirdly, Tough-Movement structures 'can be reanalysed as complex adjectival phrases, with concomitant shifts in meaning and stylistic emphasis' (p. 71).

Further characteristics of the four stylistic options are also interesting, but of special note is that 'What such examples prove is that the extraposed infinitival subject clause is the unmarked structural option' (p. 71).

Finally, Mair discusses modality and the difference between infinitive and gerundial subject clauses. It was shown that *for*-infinitival clauses do not occur with statements of truth or fact, but rather allow the speaker to avoid any commitment as to whether the situation is to be considered a fact or not. However, plain *to*-infinitival clauses and gerundial clauses are modally neutral and can be used in the environment of any modality. Rather, the difference between using an infinitival subject and a gerund is determined by extraposition.

> Nonfinite subject clauses will normally be extraposed and infinitival, but if a subject clause largely contains given information or strong anaphoric links to the preceding context, then extraposition is less likely and the clause will normally be realised as a gerundial construction (p. 88).

Chapter 3 focuses on *to*-infinitival complement clauses which are complements of transitive verbs, divided into four types:

(a) Infinitival clauses in monotransitive (SVO) patterns—without overt subjects:
  (1) 'I am attempting *to catch up a little on the Hardy Boys*.' —with 'raised' subjects;
  (2) 'The gov't in Delhi wants *the fighting to end*.'—*for*-infinitival object clauses;
  (3) 'I will arrange *for your mother to sign them when she comes*.'

(b) Infinitival clauses in ditransitive (SVOO) patterns,
  (4) 'I thought I had *asked you to restrict your drawings as far as possible*.'

(c) Infinitival clauses in complex transitive (SVOC and SVOA) patterns
  (5) 'The worthy citizen stays to face the plague because he believes *it to be God's will*...'
  (6) 'A lot of money is needed to increase the size of Colfox *to take the extra pupils*...'; and

(d) Elliptical adverbial infinites of purpose and result.
(7) 'I gave him a small amount of Equagesic *to try*.'

Again, surveying the data and dealing with each case rather exhaustively with references to the analyses of others, Mair's conclusions, benefiting from a larger discourse corpus and correlations between syntactic form and communicative function, are not simple:

> One particular syntactic structure never has one particular communicative function. Rather, its functional potential is flexibly defined by a complex network of structural, semantic, pragmatic and psychological factors (p. 222).

Each of these factors is defined and illustrated from the data. Mair analyzes each kind of infinitival complement clause, according to the behaviors of their various matrix verbs, finding considerable overlap and fuzzy borders between some of the different kinds, and saying:

> If the analysis of infinitival complement clauses in the Survey corpus has shown one thing, it is the pervasiveness of gradience, and of trade-offs between syntactic 'correctness' and optimal efficiency in the communicative situation. Factors besides the matrix verb need to be taken into account in order to explain why a particular structure of complementation was chosen in a given instance. Of course, this extension of the analyst's scope makes syntactic investigations more difficult, and their results less gratifyingly exact. It must not be forgotten, however, that fuzziness in the definition of grammatical categories and gradient transitions between them are problems only for the linguist attempting a formalised description of a supposedly autonomous syntactic component of a human language. In terms of language use, flexibility in syntactic coding is an indispensable asset (p. 220).

It is very clear that flexibility and fuzziness, in this case, are not the results of careless analysis, but rather of a thorough analysis committed to discovering what really makes the language tick, rather than to reducing it to a minimal possible underlying formal structure.

The stated scope of this work is British English, though some interesting differences from American English are noted as well. Had it been an American publication, it might well have been titled, *All about Infinitival Clauses*. It is thoroughly scholarly and exhaustive, with far more detail and technical terminology than I really wanted to

know. I had difficulty in digging out the meat in the first or even second reading and could wish that the ongoing discussions over being corpus-based and functional rather than limited to formal syntactic analysis could have been relegated to footnotes at the bottom of the pages. Instead, it was a running mix. On the other hand, a scholar in the midst of the fray would not find it such difficult going and would as well have all three goals treated together, as they are.

I was disappointed that this was not a 'discourse' study, as I view discourse; but I must admit that Mair handles the discourse context well and to good advantage. One cannot but be impressed by the author's expertise in the whole gamut of syntax, semantics, and pragmatics. While infinitival complement clauses are sufficiently complex to warrant further analyses and debates for years to come, I believe that this volume will always be considered to be, at least, one of the most definitive studies available.

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ROUNDTABLE: Computer training for linguists.

Pursuing the line of thought suggested by Geoffrey Hunt in the following article, the editors of NL are open to beginning a short ROUNDTABLE series on the subject of "Computer Training for Linguists." We invite brief (1 to 3 page) articles to be submitted by instruction staff and students about their experiences with teaching or learning linguistic applications for computers in the setting of linguistic classes or workshops. Articles may discuss such things as teaching goals, classroom technique, or evaluations of specific computer programs. This is your chance to speak out on what is effective in learning to use computers as a linguistic tool.

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Coordinator's Corner

New Light

The treatment of suprasegmentals has in some ways come full circle over the past half century. Forty and more years ago Ken Pike and Eunice Pike were analyzing tone in Mexican languages as essentially independent of the syllables they reside on or in. It was not simply their preference as a way of handling tone: the nature of tone behaviors in those languages forced them to conclude that tones and segments needed to be treated independently. We might say that today Autosegmental analysis has caught up with the Pikes.

If, then, independence of tone from the vocalic segments they are attached to, as analyzed in an Autosegmental approach, might be taken as not significantly different from a traditional Pikean approach, wherein lies the difference in the analyses?

With the development of Autosegmental Phonology has come the need for a good example of a description of a tone system using that model. It is difficult enough to produce an article that focuses on one or another problem; what one needs to do to present a whole complex tonal system in a description is something else. What can one do to analyze and present a progression from simple beginning to complex data patterns, to reveal the core of the system?

I think that John Daly's article on Peñoles Mixtec Tone is valuable as an answer to both of the above questions. It shows some of the basic differences between an Autosegmental analysis and a traditional analysis. It also provides a way of presenting a very complex tonal picture in a way that enables the reader to capture how the system works.

Moreover, John's article raises questions of theoretical interest, showing that not all the issues have been pinned down yet. There is more to be done.

Need a breather after going through *The role of tone Sandhi in tone analysis*? Try John Verhaar's *Linguistics as a stepchild: A diary entry*!

Data Sifting

In *What can you do with FindPhone?* David Bevan as programmer shows us his view of what FindPhone (FF) is capable of doing. We might be asked, why bother explaining in NL what a computer program can do? Answer: all too often an established orthography is challenged after crucial publications are already paid for and distributed, and the researchers for the phonology analysis are either no longer around or they have long forgotten most of the crucial data that led them to formulate the basic phonological distinctions needed in the orthography. Appropriate use of FF should not only provide new researchers with a tool for analysis: it should help archive and classify important data for future reference. You never know when you might need it.

Transition

I am glad to be able to take this opportunity to welcome Dr. David Payne as new SIL International Linguistics Coordinator and editor for Notes on Linguistics. David comes to us with a background of years of field work in Peru and a depth of experience as Director of the Oregon SIL at the University of Oregon. ■

The Role of Tone Sandhi in Tone Analysis

John Daly

A standard practice in tone analysis is to determine tone contrasts independently of insights derived from tone sandhi. The analyst follows the well-known procedure of sorting morphemes into classes determined by differences in syntactic classification, canonical shape, and pitch. Typically, one first chooses disyllabic nouns which represent each of the pitch differences and places them in a frame whose tones do not change. The nouns that form the substitution list are then compared with each other to determine tone contrasts. These contrasts are the evidence for the distinctive tones (tonemes) of the language.

After applying these and supplemental procedures, such as determining if there is downstep, the analyst proceeds to examine tone sandhi. Tone sandhi is analyzed by determining the changes from one toneme to another in particular environments. The tonemes determined by the basic procedures serve in the description of tone sandhi without the analyst questioning the basic contrasts in the light of tone alternations.[1]

My thesis is that proceeding in tone analysis in this way can lead to very different results than if one takes morphophonemic considerations into account in determining pitch contrasts. This is not to question the value of the procedures of sorting data and determining pitch differences. The procedures are extremely useful in doing tone analysis, but it is my contention that using these procedures alone in determining tone contrasts can lead to a serious distortion of the tone system.

From experience I have found the necessity of making morphophonemic considerations fundamental to arriving at a satisfactory phonemicization of tone. In Peñoles Mixtec (PM), the language which has been my major project since 1957, I came up with a phonemicization of tone that led to a complex description of the morphophonemics of tone, and it was only after radically reanalyzing tone contrasts in the light of tone sandhi that I achieved a basis for a

relatively simple description of the morphophonemic system (Daly 1977).

In this paper my objective is to demonstrate in the analysis of PM the different results that are obtained by following the two methods of tone analysis. This is an informal presentation of just enough of the tone system from the two perspectives to demonstrate the need to approach the determination of tone contrasts in the light of tone sandhi. A comprehensive description of the tone system is in preparation following a variety of autosegmental phonology that incorporates innovations which I find useful for an insightful description of the tone system.[2]

I need to clarify at the outset that I do not adopt the outdated point of view that a linguistic goal is to determine procedures that can be applied mechanically to achieve an adequate analysis. Surely there is no linguist today who holds this position. We recognize that procedures are useful but that there is no substitute for the creative work of the analyst. Furthermore, it is probably the case that no one would insist that the only way to do tone analysis is to determine tone contrasts independently of morphophonemic considerations; any procedure which helps to get satisfactory results is legitimate to follow. Even though we may be in agreement on this point, the fact is that in practice we do not take advantage of all the resources available to us.

The approach I insist on in the analysis of tone is not at all novel in the analysis of phonological segments. In generative studies it almost goes without saying that morphophonemic alternations are basic to determining underlying phonological forms. This approach, however, has not been adopted in many tone studies.

The rest of this paper is in two parts. In the first part are the results of approaching the analysis of PM tone by placing nouns in frames to demonstrate the contrast between tones. After determining the tone contrasts, the analyst assigns tones to each of the distinctive tone patterns of the nouns and subsequently describes morphophonemic alternations.[3]

In the second part of the paper are the results of approaching the analysis by examining the pattern of morphophonemic alternations before deciding what the tone contrasts are. Recognizing the

regularity in these alternations leads to a phonemicization of tone that simplifies the account of tone morphophonemics. The phonemic distinctions posited in this way are then defined phonetically. This does not mean that the analysis of the phonetics of tone is left until last. The analytical process is an interaction between attempts at finding a simple description of the morphophonemics of tone and finding a convincing and relatively simple description of the phonetic value of tone.

1. Analysis of PM tone via contrastive sets

1.1 Basic tone patterns

There are eight classes of disyllabic nouns that differ by tone. Two of these classes are further subdivided by their effect in sandhi on following tones, but this further distinction need not enter into determining the tone contrasts. Placing the eight classes into various frames shows that there are three contrastive tones, Low (L), Mid (M) and High (H).

The procedure followed is to place a representative of each of the eight classes of nouns into a frame which gives reference points for the level of the tones of the substitution items. A frame (with its substitution items that show the tone contrasts) forms what is known as a contrastive set (E. Pike 1992). Following a frame that goes from low pitch on the first vowel, to a higher pitch on the second vowel and to a higher pitch yet on the third vowel, the three tones H, M, and L and a glide from M to L contrast with each other.

(1)

Frame	Substitution Items		
L M H ni?i -de [_ ¯ ¯]	L H tata [¯ ˊ]	CON.find -he 'he finds father'	father
	L M čiba [_ ¯]		goat
	L L kɨtɨ [_ ˎ]		animal

L ML
ñɨʔɨ́ sweat house
[_ \ ]

Three phonemic tones will account for the contrast on the last vowel of the first three substitution items. The second vowel of the word for 'father' is at the highest level so is assigned a H tone. The second vowel of the word for 'goat' is at an intermediate level so is assigned a M tone. The second vowel of the word for 'animal' is at the lowest level so is assigned a L tone. The glide on the second vowel of the word for 'sweat house' glides from the mid level to the lowest level so is interpreted as a glide from M to L. Comparing the tones of the substitution list with those of the frame suggests that the tones of the frame are L M H.

The tone of the first vowel of each of the substitution items is essentially the same, except that the tone of the first vowel of the word for 'father' may be an upward glide or it may fluctuate to a level tone. When the glide occurs it is most naturally interpreted as a subphonemic glide forming a transition from a L tone on the first vowel to a H tone on the second vowel.

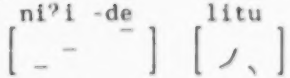
Another contrastive set is the same L M H frame followed by two nouns that begin at high pitch level. In this set the tones H and L contrast following a H tone.[4]

2) L M H H H
ni?i -de dutu priest
[_ - ¯] [¯ ¯]
[_]

H L
sanu daughter-in-law
[¯]
[\ ]

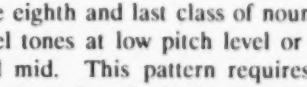
Six of the eight tone classes of nouns have the tone patterns shown in (1) and (2). A seventh class, seen in (3), is most naturally analyzed as having a LM glide on the first vowel and a L tone on the second vowel.

(3) L M H LM L
ni?i -de litu pitcher



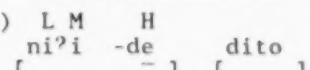
The eighth and last class of nouns, seen in (4), has the pattern of two level tones at low pitch level or at a level intermediate between low and mid. This pattern requires special treatment so no tones are shown on the substitution item as yet.

(4) L M H
ni?i -de dito uncle



Morphemes of this tone class show downstepping. Successive instances of the same pattern are on successively lower levels.

(5) kini dito didi see uncle aunt
'the uncle sees the aunt'



A reasonable analysis of this phenomenon is to assign to each morpheme of this class two H tones and to place before them a L tone (symbolized as (L)) which is not itself actualized but causes the lowering of the following H tones. Thus the tones of each of the nouns in (4) and (5) are (L)H H.[5]

There is support for this analysis from the lowering of H tones following actually occurring L tones, as seen in (6). The noun for 'priest' has two H tones that are lowered to the level of the preceding L tone.

(6) H L H H
li?i dutu rooster priest
'the priest's rooster'

Furthermore, morphemes with the tone pattern (L)H H are identical tonally to morphemes with the pattern H H in this same context. The neutralization of the tones of these two classes of morphemes arises from their both having the pattern H H following a L tone. Compare (6) with (7).

(7) H L (L)H H
li?i dito
[– – – –]
rooster uncle
'the uncle's rooster'

Where the two classes contrast, it is because one has a (L) tone and the other does not, illustrated in (8).

(8) a. H H H H
uni dutu
[– – – –]
three priest
'three priests'

b. H H (L)H H
uni dito
[– – – –]
three uncle
'three uncles'

Thus far we have found evidence for 5 tone patterns of one tone to a vowel in disyllabic morphemes. They are L H, L M, L L, H H, and H L. In addition there is the pattern (L)H H and two patterns with glides analyzed as sequences of two tones, L ML and LM L. These eight patterns are what I consider to be the basic tones of the eight classes of nouns in an analysis based on contrastive sets.

1.2 Derived tone patterns

Three derived tone patterns occur following a L ML morpheme. (The final L tone of L ML is lost whenever it occurs before a M tone.)

(9) LM M H
čiuⁿ tata
[–] [– –]
work father
'father's work'

M M
dito
[– –]
uncle

M L
sanu
[– ˎ]
daughter-in-law

In (9) the tones H, M, and L contrast following a M tone in the patterns M H, M M, and M L.

An additional pattern arises in tone sandhi. The pattern H L when followed by the enclitic for 'third person plural' becomes H M.

(10) L M H H L L → daughter-in-law –their
ni?i -de sanu -žu

L M H H M L
ni?i -de sanu -žu
[_ ¯ ¯] [¯ _ ↘]

Contrasting this derived pattern with two basic patterns gives additional evidence for the three tones H, M, and L. Compare the second tone of each of the substitution items in (10) and (11).

(11) L M H H H L
ni?i -de dutu -ši priest –her
[_ ¯ ¯] [¯ ¯ ↘]

H L L
sanu -ši daughter-in-law –her
[¯ _ ↘]

We now have evidence for all nine possible patterns of one tone to a vowel on disyllabic nouns: H H, H M, H L, M H, M M, M L, L H, L M, and L L. In addition there are the three patterns: (L)H H, LM L, and L ML.

1.3 Tone patterns in sandhi

With the tones established for each of the eight classes of nouns, we are now ready to look at tone changes which are conditioned by the environment to the left of the nouns. Following a subclass of L L morphemes, the changes in tone patterns are:

(12) H H → L H
(L)H H → L M
H L → LM L
L L → L ML

In (12) each of the four patterns which undergoes change is one of the eight tone patterns which I consider to be basic. These four patterns turn into four other patterns which I also consider basic. In other words, four of the eight patterns fall together with the other

four patterns, making a total of four tone patterns in this one environment.

Following the pattern L ML, the changes in tone patterns are:

(13)

H H	→	M H
L H	→	M H
(L)H H	→	M M
L M	→	M M
H L	→	M L
LM L	→	M L
L ML	→	L L
L L	→	(unchanged)

In (13) one pair of tone patterns becomes M H, a second pair becomes M M, a third pair becomes M L and a fourth pair becomes L L. It is curious that the members of each pair of patterns which become alike in (13) are patterns which are on the left and right sides of the same arrow in (12). Notice, for example in (12), that H H morphemes become L H. Next notice that in (13) these two patterns, H H and L H, become M H. A comparable generalization is true of each pair of tone patterns: H L becomes LM L in (12), and these two patterns become M L in (13), etc. One can hardly be content with the analysis as it is; surely some underlying similarities are being obscured.

A further difficulty is the lack of generality in the tone changes. For the changes in (12), little is gained by attempting to formulate rules; one might just as well simply list the changes. For the changes in (13), although each pair of tone patterns which becomes alike can be brought together in a rule, the changes are very irregular. No system of changes is apparent, and if one thinks that a tone analysis should reflect what the native language learner acquires, it stretches the imagination to think that the language learner has internalized anything like the complexities of this system.

One can try to make minor changes in the analysis that I have developed up to now without much success. Something more radical is needed.

2. Analysis of PM tone drawing on insights from tone sandhi

2.1 Basic and derived tone patterns

In a reanalysis of the tone contrasts that arise from a consideration of tone sandhi, I posit two tones, L and H, and a feature of high register (h). The feature of h register in underlying structure is only associated with a L tone (which I call a h register L tone (L^h)). As we will see, the phonetic effects of h register are not in the L tone itself but in following tones that are actualized at a higher phonetic level. Register h and the two tones L and H combine to give the eight basic tone patterns:

H H, H L, L H, L L, L^hH H, L^hH L, L^h H and L^h L.

Following a subclass of L L morphemes, the changes in tone patterns are:

(14)

H H	→	L^hH H
H L	→	L^hH L
L H	→	L^h H
L L	→	L^h L

In (14) the first tone in each of the derived patterns is a L^h tone. This tone occurs before a H tone on the same vowel, or if there is no H tone, it is the only tone on the vowel. I account for this change by associating a register h at the beginning of the first vowel of each morpheme, and subsequently introducing a default L. (Default L is also introduced on every vowel which has no tone, i.e. has neither a H tone nor a L^h tone.)

Following the tone pattern L^h L, the changes in tone patterns are:

(15)

L^hH H	→	H H
L^hH L	→	H L
L^h H	→	L H
L^h L	→	L L

In (15) the register h of each morpheme on the left is deleted. (Default L is not yet present so it is not necessary to delete a L tone before a H tone on the same vowel.)

The two kinds of morphophonemic change one following a subclass of L L morphemes and the other following the tone pattern L^h L, are summarized in (16).

(16)

H H	↔	L^hH H
H L	↔	L^hH L
L H	↔	L^h H
L L	↔	L^h L

The arrow pointing in both directions represents the two directions of change. Following a subclass of L L morphemes, the tones on the left become the tones on the right, and following the tone pattern L^h L, the tones on the right become the tones on the left. Four basic tone patterns in one environment change into the other four tone patterns. The second four basic tone patterns in another environment change into the first four patterns. In each change it is the same pair of tone patterns that change from one to the other. The individual tone changes are simply the result of the association of a register h following a subclass of L L morphemes, or the deletion of a register h following L^h L. Default L is subsequently introduced.[6]

The new statement of morphophonemic change is considerably simpler than the one given in the previous three tone analysis. The greater simplicity is more than anything due to recognizing that some previously analyzed M tones are a lower allotone of H tone (and others are L tones). A barrier to gaining this insight is that the pitch difference between the lower allotone of H and the higher allotone of H is as great as or greater than the difference between H and L tones.

2.2 Phonetic value of tone

The next task is to attempt to give a convincing statement of the phonetic value of tone. In both the old and the new analyses, phrase final L tones glide to extra low, one or more H tones are at a lower level following a L tone, and a L tone is lower than a preceding H tone. In this brief attempt at justifying a different approach to the analysis of tone, I do not go further into level sequences of tones or tones on successively lower levels inasmuch as both approaches face similar challenges.[7]

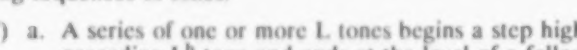
In the analysis based on tone sandhi, two additional statements need to be made on the phonetic value of tone, both of which apply to rising sequences of tones.

(17) a. A series of one or more L tones begins a step higher than a preceding L^h tone and ends at the level of a following H tone or at extra low pitch level before pause.

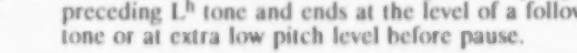
b. A H tone is a step higher than a preceding L^h tone, and additional H tones are a step higher yet. (Any number of L tones may intervene between the L^h tone and the H tones.)

The phonetic value of L tone as defined in (17a) is illustrated in (18).

(18) a. L^h L L^h L L L^h L L L



b. L^h L H L^h L L H L^h L L L H

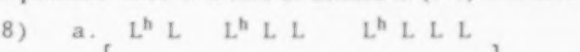


In (18a) a L^h tone occurs before one or more L tones. The L tones begin a step up from the L^h tone and end at extra low pitch at the end of a phonological phrase.

In (18b) a L^h tone occurs before one or more L tones plus a H tone. The L tones begin a step up from the L^h and end at the level of the H tone, which is also one step up from the L^h tone. Thus, the L tones and the H tone are on the same level.

The phonetic value of H tone as defined in (17b) is illustrated in (19).

(19) a. L^h H L^h L H L^h L L H



b. L^h H H L^h L H H L^h L L H H L^h H H H

In (19a) and (19b) a H tone immediately following a L^h tone, or following a L^h tone plus any number of L tones, is one step up from the L^h.

In (19b) all but the first H tone are an additional step up. Any number of L tones may intervene between the L^h tone and the H tones without affecting the level of the H tones.

To see how the two additional statements on the phonetic value of tone account for the contrastive sets, take first the contrastive set in (1) above, where the three tone frame was thought to be L M H and is now L^h H H.

(20)

L^h H	H	L^hH H		
ni?i	-de	tata	CON. find	-he father
			'he finds father'	

L^h H	
çiba	goat
L L	
kɨtɨ	animal
L^h L	
ñɨ?ɨ	sweat house

The L^h tone of the frame in (20) is at low pitch level and occurs before a H tone, one step up, and by a second H tone, an additional step up. The same tone pattern in the frame also appears in the noun for 'father', which has the two tones L^h and H on its first vowel, accounting for the glide from low pitch level to a level a step higher. The H tone on the second vowel follows L^hH so is a step higher yet. The noun for 'goat' has a L^h tone at low pitch followed by a H tone a step up. The tones of the noun for 'animal' have the same values as in the preceding analysis: the two L tones begin at low and end at extra low. The noun for 'sweat house' has a L^h tone at low pitch followed by a L tone that begins a step up and ends at extra low.

The tones of the substitution items of the contrastive set in (2) above are the same as in the previous analysis.

(21) L^h H H H H
ni?i -de dutu priest
[_ ¯ ¯] [¯ ¯]

H L
sanu daughter-in-law
[¯ ˎ]

Turning to the substitution item in (3) above, instead of having a glide consisting of the tones L and M on the first vowel, it has a glide consisting of the tones L^h and H.

(22) L^h H H L^hH L
ni?i -de litu pitcher
[_ ¯ ¯] [ˊ ˎ]

The sequence of downstepped disyllabic forms cited in (5) above consists of alternating L and H tones in the reanalysis. A H tone is at the same level as an immediately preceding L tone, and a L tone is at a lower level than an immediately preceding H tone. A representation of the sequence of three nouns is:

(23) L H L H L H
kini dito didi 'the uncle sees the aunt'
[_ ¯ _ ¯ _ ¯]

In (24) I bring together diagrams of the phonetic values of the eight basic tone patterns seen in the contrastive sets above, relating them as they are in tone sandhi. The tones on the left become the tones on the right following a subclass of L L morphemes. The tones on the right become the tones on the left following the tone pattern L^h L.

(24) a. H H ↔ a'. L^hH H
[_ ¯] [ˊ ¯]

b. H L ↔ b'. L^hH L
[¯ ˎ] [ˊ ˎ]

c. L H ↔ c'. L^h H

[– –] [_ –]

d. L L ↔ d'. L^h L

[– ˎ] [_ \ ]

In the original analysis of the contrastive set in (9) above, each substitution item undergoes a change from one phonemic tone to another. In the reanalysis of this set as shown in (25), there is no tone sandhi—the tone differences are entirely allophonic. The definitions in (17) above predict the levels of the tones following a L^h tone.

(25) L^hL H H work father

čiu^n tata 'father's work'

[_] [_ –]

L H

dito uncle

[– –]

H L

sanu daughter-in-law

[– ˎ]

In (25) the L^h L frame begins at low pitch and moves one step higher. The L tone does not glide lower because it is not a part of one or more L tones at the end of a phrase. The first H tone of the word for 'father' is at the same height as the preceding L tone, one step up from the L^h tone. The second H tone is an additional step up. The L tone of the word for 'uncle' is on the same level as the preceding L tone, and the H tone of this noun is on the same level as the preceding L tone, one step up from the initial L^h tone. The H tone of the word for 'daughter-in-law' is at the same level as the preceding L tone, and the final L tone glides to extra low.

The tone pattern of the substitution item in (10) above is now the pattern H L followed by a floating H tone that accompanies the enclitic for 'third person plural'. The floating H tone associates to the immediately preceding vowel to give the pattern H LH, where the tones L and H on a single vowel are phonetically a level tone. This

analysis is consistent with a L tone followed by a H tone on separate vowels that are also at the same level.[8]

(26) L H H H L H L →
ni?i -de sanu - žu daughter-in-law -their

L H H H LH L
ni?i -de sanu -žu

The phonetic value of tone defined and illustrated in this section accounts for all the data we have seen. In addition it lends itself to a description of optional drift of one or more L tones. Some L tones at the same level as a following H tone may optionally drift lower, especially in longer sequences of L tones and in L tones preceded by a H tone. By maintaining the phonemic distinction between L and H tones before a H tone, it is possible to specify L tones as varying between level tones and drifting tones in contrast to H tones, which never drift lower. If a distinction between H and L tones were not maintained, it would be difficult to specify which tones may optionally drift and which may not. The analysis based on contrastive sets, which postulates basic M tones and in addition turns H tones into M tones, loses the distinction between the two tones, thereby failing to give a basis for an account of optional L tone drift.

3. Summary

The analysis of tone contrasts in PM based on contrastive sets and the analysis that takes tone sandhi into account give radically different results.[9] What appears to be a contrast between M and H tones determined on the basis of contrastive sets turns out to be not a contrast between these two tones but, with one exception, the consequence of a L^h tone occurring to their left. In the reanalysis based on tone sandhi, the two tones that were thought to be in contrast are instead two allotones of a H tone, which differ from each other in pitch as much as or more than the two phonemic tones L and H differ from each other. The exception to a preceding L^h being the source of an apparent contrast between M and H is in the sequence of tones LH on a single vowel, which are at a level intermediate between H and L.

The phonemic distinctions that follow from taking tone sandhi into account lead to a radical simplification in the description of tone morphophonemics. The changes we have seen in two contexts are completely regular. In one context four basic tone patterns turn into four other basic patterns, and in a second context the latter four patterns turn into the first four. The same patterns are paired in whichever direction the change occurs. The change from one tone pattern into another is due to the addition or deletion of a register h.

A comparison of the two analyses of tone sandhi makes it clear that the analysis based on contrasts in tone sandhi is simpler and more insightful.

In the analysis based on tone sandhi, the change to be made following a subclass of L L morphemes shown in (14) above is:

> Associate register h at the beginning of a vowel (and add L by default).

In the analysis based on contrastive sets, the changes following a subclass of L L morphemes are complex:

> H before H becomes L (seen in H H becoming L H and (L)H H becoming L M);
>
> H preceded by (L)H becomes M (seen in (L)H H becoming L M);
>
> H before L becomes LM (seen in H L becoming LM L);
>
> L preceded by L becomes ML (seen in L L becoming L ML).

In the analysis based on tone sandhi, the change to be made following a L^h L morpheme shown in (15) above is:

> Delete register h at the beginning of a vowel (and add L by default).

In the analysis based on contrastive sets, the changes in tones following a L ML morpheme are:

> H or L becomes M before H (seen in H H and L H becoming M H);
>
> Each H of (L)H H becomes M (seen in (L)H H becoming M M);
>
> L becomes M before M (seen in L M becoming M M);

H and LM become M before L (seen in H L and LM L becoming M L);

ML becomes L following L (seen in L ML becoming L L).

The analysis based on contrastive sets that gives the three phonemic tones H, M, and L also proves to be inadequate in longer sequences of tones. With the tones postulated on the basis of contrastive sets, the condition for the substitution of one toneme for another at the end of a phonological phrase can be as far away as the beginning of the phrase. Whether a H tone remains H or becomes M at the end of a phonological phrase may be correlated with the presence or absence of a L tone at the beginning of the phrase. This kind of long distance effect in which one phonemic tone substitutes for another is suspect. In the analysis based on tone sandhi, the presence or absence of a L tone accompanied by the feature register h at the beginning of a phonological phrase may determine the phonetic level of a final H tone, as well as intervening tones. The change being allophonic is not nearly so surprising as if the change were phonemic.

Fortunately, the simplification in the morphophonemics of tone is not at the price of unduly complicating the phonetic value of tone. Although tones have unexpected phonetic values in rising sequences, their values are relatively simple: L tones are a step higher than a preceding L^h tone, a H tone is also a step higher than a preceding L^h, and subsequent H tones are an additional step up. In its own right, the phonetic system has an advantage in maintaining a phonemic distinction between H and L tones that are on the same level, making the optional drift of L tones easier to specify. The end result is a coherent system of tone morphophonemics and tone allophonics.

Notes

1 See E. Pike (1992) for a description of this method of tone analysis.

2 Autosegmental phonology, developed by Goldsmith (1976), and developments since then make it possible to formalize and refine my analysis in a way which was not possible in Daly (1977). Nevertheless, the informal description I give here is fundamentally the same as the description in my earlier work. In the work in preparation, the model I adopt includes tone features that specify the contrast between High and Low tones, the register features high and low which may be cumulative in their effect, underspecification of tone, a tone geometry that includes the tone root node in addition to the tone node, and the features High and

Low which combine on a single node to specify one of the allotones of High tone.

[3] In K. Pike's pioneering work *Tone Languages* (1948), he gives procedures that leave the analysis of morphophonemic alternations until after the basic tone contrasts have been determined. He gives no principled reason for this approach and in fact shows that he is open to other alternatives. In a footnote he offers an alternative analysis of Apache by Hoijer (1943), treating glides as sequences of two tones which would 'allow a simpler statement of morphology'. He credits Richard Pittman for calling this interpretation of glides to his attention.

[4] The H tones before the last H tone fluctuate to ascending tones below the highest level.

[5] The unrealized L tone is equivalent to a process phoneme in a tagmemic analysis (K. Pike 1976).

[6] I have limited the tone data to what is most relevant to showing the necessity of taking tone sandhi into account in determining tone contrasts. The analysis of tone sandhi in its entirety is more complex. For example, the association of L^h is not completely regular in all environments. Following one tone pattern it is associated to three of four tone patterns, and following another tone pattern it is associated only to a following H-toned enclitic. There is also a rule of H tone assimilation whose application is restricted by a complicated set of conditions.

[7] In the analysis that incorporates the register feature h, tones are prepared for phonetic interpretation by reassociating register h to following tones and introducing register low (l) by default. Register h is delinked from a L tone and reassociated to a following sequence of tones that consists of zero or more L tones plus zero or more H tones. Every other sequence of these tones is associated to a register feature l by default. Given the cumulative effect of l register, tones on successive l registers are on successively lower levels.

[8] There is solid evidence for the association to the left of a floating H tone. The floating H conforms to the phonetic value of H tone, whatever tones precede it. It may follow a H tone on the same vowel, where the first H tone is at one level and the second H tone is at a level a step higher, the two H tones forming a rising glide. It may follow a L^h tone on the same vowel, forming a rising glide from low pitch level to a level one step higher. It may follow the tones L^hH on the same vowel, forming a glide from low pitch level to a level two steps higher.

[9] Another approach to the reanalysis is to classify disyllabic forms into native terms and loans and into large classes and small classes. The native terms which form large classes are analyzed first, and then the analysis is extended to cover the other classes. This approach may lead to helpful

insights, but it is more needed in languages where there is little or no tone sandhi rather than where sandhi is extensive.

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. . . . . .

Consortium For Lexical Research

The folks at the Computing Research Laboratory at New Mexico State University have organized a consortium for lexical research. For a $500 annual fee member organizations can utilize their ever growing catalogue of resources for non-commercial, research purposes. S.I.L. has not yet joined the consortium, but if this short description of their resources sparks some research ideas which would make it worth while we will go to bat for you for an organizational membership for a year.

At last report the consortium has collected six major research tools and resources: a parser which can enable tagging for building a vocabulary data base, a 50,000 word English, electronic dictionary, Roget's 1911 thesaurus, tools for lexical acquisition, fonts and file format converters for several non-Greco-Roman alphabets, several concordance-type tools, and a data base of sample lexical entries. The tools r lexical acquisition involve a 'question-answering system' and a 'Common Facts Data Base' which is a semantic network.

S.I.L. will not join the consortium until someone comes forward with a useful research plan which could conceivably benefit from the use of their resources. If you come up with something, pass it by your nearest technical studies supervisor, i.e. at the working group, branch or area level, and if they judge it a project worthy of your time, send a proposal to Les Bruce at SIL, 7500 W. Camp Wisdom Rd., Dallas, TX 75236 U.S.A. ■

Linguistics as a Stepchild: A Diary Entry

John Verhaar, S.J.

I

The other day I had, for the umpteenth time, that experience we all know. Someone wanted to know what I do, and I said I was a linguist. Then, 'Oh, my! How many languages do you speak?' This usually gets me tongue-tied, but for once I had a reply: 'Oh, well, most of them I don't.' Of course, English idiom is against us: *He/she is quite a linguist* just means that person speaks a few languages—a **polyglot**, I would say.

Linguistics is supposed to be a social science, along with fields like psychology, anthropology, sociology. But these other fields have had quite a social impact. Among academic (and other) people, not professional psychologists, there are those who speak confidently about 'oedipal' problems, about 'ego weakness', 'self image', 'projection', and informal seminars at parties (especially those which are 'thrown') are offered, under the influence of fantasy-enhancing beverages, on such more esoteric things as the 'primal scream', or the blessings of 'egolessness'. Psychology has had a 'social impact' far outside the field.

Or, nonsociologists may discourse learnedly about how a 'group' differs from a 'crowd', and about 'critical theories' like that of the *Frankfurter Schule*. Anthropology, of course, concentrated, for the obvious reasons, on little-known (and thus nonwestern) cultures first, and this triggered the false idea with sociologists that their own field was primarily concerned with men in business suits, while anthropology worked on people in loin cloths—but that is now almost entirely a thing of the past.

II

Enter the linguist. No one not a linguist will, whether at parties or on other occasions, hold forth on the vicissitudes of epenthesis, object incorporation, headmarking languages, or even about such fantasies as recoverable zeros. But this is by no means because they do not

hold forth on language. They do—and it's all 19th century stuff. People commit blithely and unbeknownst to themselves the 'genetic fallacy': the illusion that a 'word' will 'mean' (now) what it 'meant' (originally). They will discern great mysteries in the provenance of words like *okay* or *hooligan*. They will praise you for being interested in 'primitive' languages, for, after all, the speakers thereof need 'development'—in my own mind I call that 'IMF'-linguistics.

A lawyer who happened to know some words of Indonesian confided to me once that that language was 'silly'—seeing that, to make a plural, they 'say a word twice'. One editor of a major Australian weekly, in reporting on the ordination of a bishop in Papua New Guinea, expressed joy that the ceremony had been in English, rather than in 'that bastardized form of English' —meaning Tok Pisin. Last month a local Dutch newspaper featured someone representing a group of searchers for second WW aircraft wrecks in the Papua New Guinea Highlands, and advised the readership that the 'Pidgin' spoken there was 'primitive', as well as the remainder of American presence there in the 40s.

Languages, according to contemporary beliefs among the highly educated are 'national languages'—other varieties of that language spoken in any such nation, are supposed to be 'dialects', to be looked at askance if (of course) tolerantly. I have on occasion remarked that 'standard' forms of languages are also 'dialects' (often social rather than geographical)—the dialect of the affluent and influential. Such a comment raises eyebrows.

What 'linguistics' means for those not of the field is 'words'. As for 'grammar', of course, everyone knows languages have 'endings'. (If some language doesn't have endings, it's not supposed to have any 'grammar'.) If you mention 'syntax', that reminds people of computer commands. Mention 'pragmatics', and people start clearing their throats.

III

Linguistics is thus a stepchild among the social sciences—there seems to be so little about it that is widely social—except normativism.

Children are supposed to have 'good grammar'. For English, that means many things: a spelling still of 14th century origin; indignation

about *It's for you and I* rather than *It's for you and me*. Or frowns about *He ain't no fool*, though, inconsistently, not about *Not here you can't!* Wonderment about Asian children saying *Yes* in reply to *Didn't she come?* meaning that she didn't ('Don't they know the difference between *yes* and *no*?'). Open season on speakers of other dialects. Worried education officials slaving away at some 'national curriculum'. Sending children into the teenage fray bearing the burden of being too stupid to handle their own language.

Linguists, of course, know better, but why do so few others? ■

What Can You Do With FindPhone?

David Bevan

INTRODUCTION

In a recent article (Hunt 1992), Geoffrey Hunt introduced readers of this publication to *FindPhone*, a computer program to assist with phonological analysis in the field. Since Hunt wrote his article in August 1991, the development of *FindPhone* has continued and been brought close to completion.

This article describes some of the things which you can do with *FindPhone*, concentrating on the use of its more advanced features. An introductory overview of the program can be found in Bevan 1993. The data I've used is from the Yoruba language spoken in West Africa and was collected by students on a British SIL field methods course.

CONTRAST AND VARIATION

The investigation of contrast and conditioned variation is the 'bread and butter' of phonological analysis in the field. *FindPhone* provides help with this in a number of ways.

The classical (and classic) method of demonstrating contrast is the isolation of minimal pairs. *FindPhone* will search your data and find all examples of minimal contrast for you.

Figure 1 shows some of the results from a search for words showing minimal contrast between velar and glottal phones. The results are divided into groups showing minimal contrast. (*FindPhone* calls these groups 'bundles'.) Note that in the last group, one of the purported examples of minimal contrast is probably in reality a case of free variation.

[g] gū long (Y280)
[k] kū full (Y266)
[g] agɑ chair (Y018)
[gb] agba old (Y134)
[kp] akpa arm (Y133)
[gb] agbɔ̄ chin (Y040)
[gbʷ] agbʷɔ̄ chin (Y040a)
[gbʷ] agbʷɔ̄ coconut (Y305)
[h] ahɔ̄ tongue (Y037)

Figure 1

As well as finding minimal pairs, *FindPhone* can also look for homophones. This is useful not only for finding homophonous forms, but also for locating data that you have accidentally entered twice. With a tone language, the homophones option can be used to find data that shows minimal contrast for pitch. Figure 2 shows some of the results from such a search.

Figure 2

To demonstrate conditioned variation, it is necessary to discover in what environments each phone occurs. You can achieve this with *FindPhone* by performing simple searches.

Figure 3 shows the results from a search to find the environments in which the voiceless dental affricate occurs. The results suggest that its occurrence is restricted to environments in which it is followed by a close front vowel.

```
ʦ/ɐ_i  ɐʦi      ear
ʦ/ɯ_i  ʦiʦi     road
ʦ/ɪ_i  ʦiʦi     road
ʦ/ɯ_i  ʦiʧobi   big
ʦ/ɯ_ɪ  ʦiʧiɪ    thin
ʦ/o_i  ʤoʦi     dirty
```

Figure 3

FindPhone enables you to display contrast and conditioned variation by creating **contrastive oppostion charts** (CO charts). To make a CO chart, you set up a class containing the phones you are interested in and execute a search using one search pattern for each of the relevant environments.

Figure 4 shows typical search settings for creating a CO chart. In this example, *I* is a class of close front vowels, *A* is a class of open vowels, *<<-CI>>* is a temporary class of vowels not in *I* and *<<-CIA>>* is a temporary class of vowels in neither *I* nor *A*.

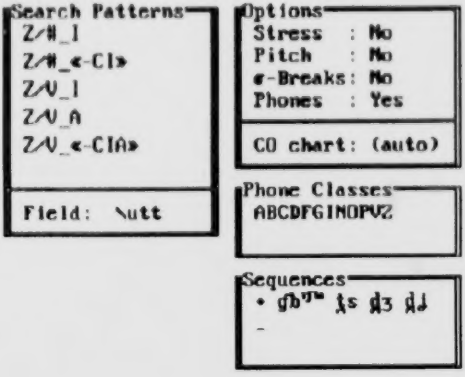

Figure 4

Searching the data using these settings produces the chart in figure 5. Each phone has its own column and each environment has its own set of rows.

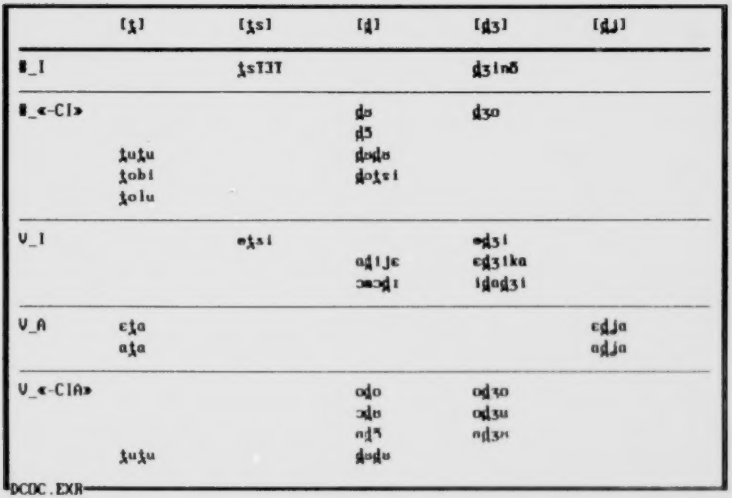

Figure 5

Contrast is shown by two words being lined up horizontally. Note that the contrast is not necessarily between phones in identical environments; *FindPhone* is able to find examples of contrast in similar or analogous environments.

Conditioned variation is shown by two phones never occurring in the same environment. For example, the chart shows that the voiceless dental plosive and the voiceless dental affricate are in conditioned variation, conditioned by the presence or absence of a following close front vowel.

FindPhone can save you a lot of time in searching your data for examples to demonstrate contrast. It also makes checking for conditioned variation relatively easy. However, it is still the user's responsibility to choose meaningful environments when creating CO charts.

INTERPRETATION

When doing phonological analysis, one of the problems (the so-called unit-sequence interpretation problem) is to determine whether a

sequence of phones such as a plosive followed by a homorganic fricative or a nasal followed by a homorganic plosive should be considered a single phonological unit (e.g. an affricate or pre-nasalized plosive) or a sequence of two or more units. Before you have interpreted your data, it is important that your options are kept open. *FindPhone* enables you to do this by letting you change your interpretation from search to search.

Figure 6 shows the *FindPhone* Ambivalent Sequences box. It contains four sequences. With the settings as shown, the program will treat the first as a single unit (a labial-velar double plosive with nasal release), whereas the others will be treated as sequences of two units. As well as making it easy to change your interpretation between searches, the ambivalent sequences box saves you from having to join affricates with a ligature in your data.

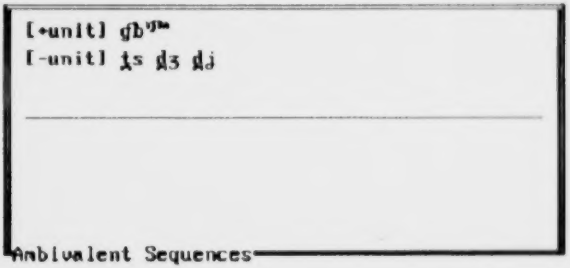

Figure 6

VOWEL HARMONY

Vowel harmony and similar non-linear phenomena are found in many languages. *FindPhone* enables you to study such language features by using its built-in consistent changes option to create new data fields which you can then search.

In investigating vowel harmony, what is required is a field containing just the vowels. To create such a field with marker *\voh*, you use the search settings shown in Figure 7. The search pattern simply removes consonants from the data.

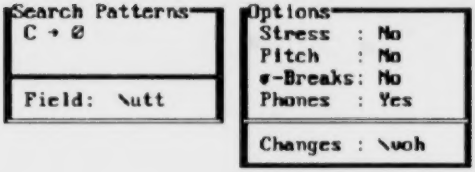

Figure 7

Figure 8 shows the data after adding the new fields. Note that the vowels have been kept lined up with the pitch contours so that the vowel harmony field can be searched together with the pitch field if required.

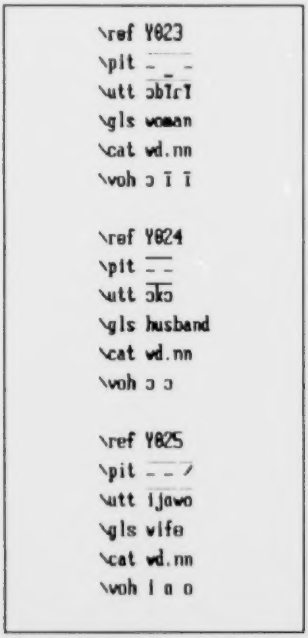

Figure 8

You can now search the *voh* field for vowel clusters to investigate vowel harmony. For example, if *O* is a class of oral vowels, you can

use the search settings in Figure 9 to find words in which an oral vowel follows a nasalized vowel (separated by any number of consonants). In fact, there are no such words in the data used to prepare this article. This suggests quite a strong hypothesis concerning Yoruba nasalization (with a very satisfying autosegmental representation) that could be tested with further data.

Figure 9

You might find it useful to have the vowel harmony fields present all the time. To do this you would need to recreate them every time you modified your data. With *FindPhone*, you can automate this process by creating a suitable macro file (known as a ***GO file***).

PHONEMICIZING YOUR DATA

After you have determined the phonemic inventory of the language, you might want to phonemicize or orthographicize your data which can then become the basis of your lexicon. *FindPhone*'s consistent changes facility can be used to do this.

Figure 10, which shows the whole *FindPhone* screen, exhibits one possible way to phonemicize the Yoruba data, creating a new field with marker *phm*. Note that you have the choice of whether or not to make the new field line up phone by phone with the field being searched.

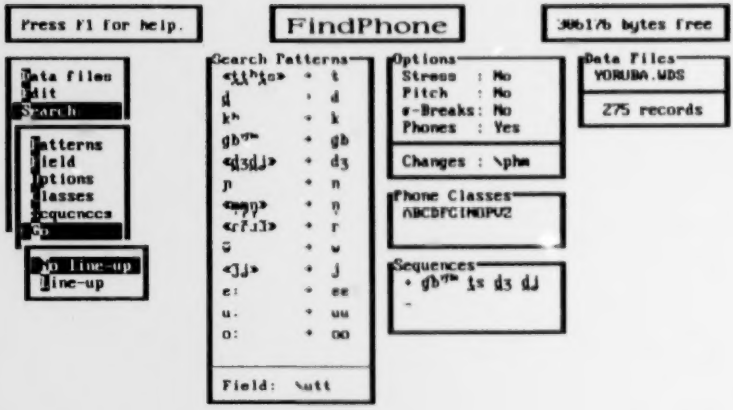

Figure 10

Figure 11 shows some of the results with the phonetic form in the middle and the phonemicized form at the left. *FindPhone*'s consistent changes feature can be used in a similar way to orthographicize your data or to create fields containing syllable structure information or CV patterns. It could also be used for testing some types of morphophonemic hypothesis by implementing generative rules.

lɑsiko	lɑsikʰo	time
ɑrugɓo	ɑrugɓo	old
lodʒidʒi	lod̪ʒid̪ʒi	suddenly
ijulɛ	ijulɛ	wife (first)
titobi	t̪sit̪obi	big
kɛkɛrɛ	kɛkɛ.ɪɛ	small

Figure 11

IS IT FRIENDLY?

In his article, Hunt (1992:5) outlines some factors which make a computer program 'friendly'. How does *FindPhone* match up?

Figure 10 shows the main *FindPhone* screen. The normal way in which you use the program is to repeatedly search your data. For this reason, the main screen displays the current search settings. At the left of the screen are the menus you use to control the program.

At the top left corner is the message 'Press F1 for help.'. You can press the *F1* key at any time and get a relevant help screen explaining what you can do at that point in the program. As well as these **usage** screens there are **error message** screens which give more information about an error message and **general information** screens that describe some aspect of the program. Altogether there are about 230 help screens.

Figure 12 shows the Phone Classes general information help screen. Note that there are links to related help topics. For example, from this screen, if you press the *A* key, you will jump to the Ambivalent Sequences help screen and if you press the *S* key, you will jump to the Search Patterns help screen.

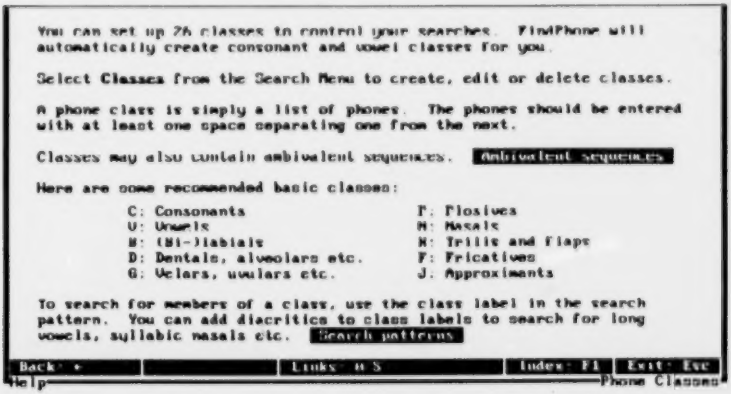
You can set up 26 classes to control your searches. FindPhone will automatically create consonant and vowel classes for you.
Select Classes from the Search Menu to create, edit or delete classes.
A phone class is simply a list of phones. The phones should be entered with at least one space separating one from the next.
Classes may also contain ambivalent sequences. Ambivalent Sequences
Here are some recommended basic classes:
C: Consonants
V: Vowels
B: (Bi-)labials
D: Dentals, alveolars etc.
G: Velars, uvulars etc.
P: Plosives
M: Masals
R: Trills and Flaps
F: Fricatives
J: Approximants
To search for members of a class, use the class label in the search pattern. You can add diacritics to class labels to search for long vowels, syllabic masals etc. Search patterns
Back: ←
Links: ↑ ↓
Index: F1
Exit: Esc
Help
Phone Classes

Figure 12

To make entering data easier, *FindPhone* incorporates a small glossary that can be used to store frequently used text or phonetic symbols. The glossary can be accessed from anywhere in the program.

BEST COPY AVAILABLE

Figure 13 shows a suitable glossary for use with the Yoruba data. For example, when editing, pressing ***Ctrl+M*** inserts the labial-velar double plosive with nasal release and saves ten keystrokes.

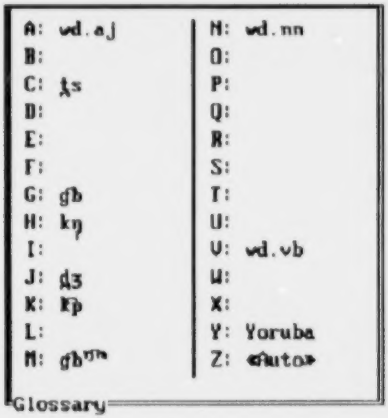

Figure 13

After executing a search, *FindPhone* makes it easy to display the results in the way you want. You simply enter the field **markers** in the Layout box where you want the field **contents** to be. Surrounding text can also be added.

Figure 14 shows the Layout box used to lay out the minimal pairs shown in Figure 1. Note that the pitch contour is positioned above the utterance. Note also the brackets around the contrasting phone and the parentheses around the reference number.

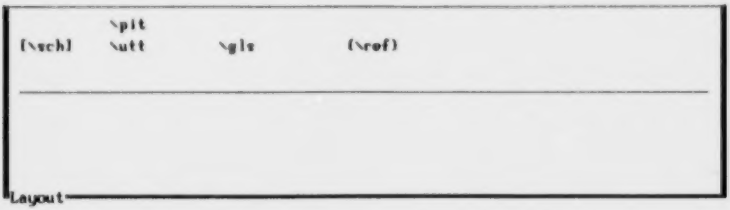

Figure 14

AVAILABILITY

FindPhone version 5.7 is available with 62 pages of documentation from the SIL Software Library on disks DOSSIL054C and DOSSIL055C for $6.00. A version with a printed manual should become available sometime during 1993.

FindPhone can be translated into other languages. This involves the translation of the program's 600 or so screen messages which are stored in a message file. If you want to produce a foreign language version of *FindPhone*, you need a copy of the *FindPhone* Foreign Language Message Support disk which contains a file containing the English messages and versions of the program which support a message file.

References

Bevan, David. 1993. Introducing FindPhone. Notes on Computing, forthcoming.

Hunt, Geoffrey. 1992. A good phonology program. Notes on Linguistics 57.4-10. ■

22nd Colloquium on African Languages and Linguistics

University of Leiden
August 30 – September 2, 1992

Robert Carlson

The 22nd Annual Colloquium on African Languages and Linguistics in the Netherlands was attended by about sixty linguists. A total of 34 papers were given by participants from Africa, Europe, and North America. As always in such conferences, there were a couple of off-the-wall papers (one purporting to reconstruct the origins of an extinct Khoisan language /Xam—and not just proto-/Xam, but the original grunts and the groans of pre-language—from a 12,000 page manuscript from the middle of the 19th century). The great majority of papers, for which such exercises provided (unintended) comic relief, were serious, however. As is usual in such conferences, most of the papers concerned matters which were of interest to linguists mostly working on African languages. Below I mention five which I found interesting.

'Inalienability in two Bantu languages' by Anna Gavarro, University of Barcelona, examined, in a GB framework, a special kind of 'double object' construction in Haya and Sesotho in which the two direct objects are semantically possessor and inalienably possessed. These particular double objects do not behave with respect to rules like ordinary double objects (in which the semantic roles are typically patient and recipient). According to Gavarro, this means that the configuration of the verb and the two NPs within the VP must be asymmetric, with the possessor C-commanding the possessed. Apparently the only way to obtain this desired configuration is by assuming that the verb is in fact 'underlyingly final', although it is never final on the surface. This proposal was met with skepticism by most of the Bantuists present, though it should be mentioned that few of them are working in the GB model.

'Head-marking a third argument in Somali' by John Saeed, Trinity College, Dublin, raised (without solving) an interesting problem in

Somali case marking. Somali indexes subjects and objects with clitics in the 'verbal piece'. Subject marking is unambiguous, but up to two objects can be marked, using two different series of object clitics. The problem is that when a second is added (the 'third argument' of the title), neither the position of the NP in the clause, nor the choice of object clitic, tells anything about the different semantic roles of the two participants involved. Thus the same sentence can mean, e.g., "They took us from you" or "They took you from us". Presumably speakers of Somali use discourse-pragmatic strategies to disambiguate such sentences, but the research remains to be done.

'The noun prefixes of new Benue-Congo' by Kay Williamson, University of Port Harcourt, adds some interesting data from Igboid languages to the effort to reconstruct the noun class prefixes of proto-Benue-Congo. Until recently these languages were not included in Benue-Congo. Now that they are, they are seen to provide some interesting new solutions to old problems. What is surprising about this is that the Igboid languages no longer have functioning noun class systems. Nouns do, however, have remnants of class prefixes which are synchronically part of the root. The bit of interesting data these ex-suffixes provide is actually simply a downstep in the tone register which occurs with some nouns and not with others. According to Williamson, this downstep is the reflex of an old low-tone prefix. She shows that in most branches of Benue-Congo nouns actually had two prefixes—one is the prefix inherited from proto-Niger-Congo; the other is an 'augment', apparently originally functioning as some sort of determiner. She traces the reflexes of this combination of augment + prefix in the various branches of Benue-Congo. Of particular interest is the suggestion that the proto-prefix for classes 9 and 10 was in fact a low-tone *n-, the *i- prefix reconstructed by de Wolf (1971) being in fact the augment. Miehe (1991) has shown that these nasal prefixes can be reconstructed for proto-Niger-Congo.

'A new language atlas of Nigeria' by Roger Blench, provoked intense interest among the Nigerians and those doing research on Nigerian languages. The atlas, which is to be published by SIL in cooperation with the University of Ilorin, is a significant improvement on the Index of Nigerian Languages of 1976. It increases the number of languages listed from 394 to 440. A number of languages are reclassified (the biggest change being the extension of Benue-Congo

noted in the previous paragraph), and the bibliography updated. The map accompanying the atlas is completely computerized, and it is hoped that a 'running edition' can be kept in the coming years.

'Basic odor terms in Bantu' by Jean-Marie Hombert, Universite Lumiere-Lyon, is a first attempt to do for smells what Berlin and Kay did for colors. Hombert speculated that languages spoken by hunter-gatherer cultures may have a larger repertoire of 'basic odor terms' (using the same criteria for deciding if a term is 'basic' that Berlin and Kay used) than Indo-European languages because odors may be more significant to them. He investigated several zone b Bantu languages of Gabon and found that this was indeed the case—the languages had from eight to eleven basic odor terms from the following repertoire: dirty (sweaty) person, urine, fish, fresh guts, burned hair, pepper, civet cat, rotten things, game animal, chicken excrement, fried food, moldy/musty. What makes the investigation of odors more problematic than that of colors or sounds is the fact that the perceptual space is not well defined. There is nothing like wave-length of light or frequency and amplitude of sound with which to correlate the physiological phenomena. Which molecules cause which odor is a complex and little-understood matter. Moreover, specific anosmia (inability to perceive certain odors—analogous to color-blindness) is quite frequent, affecting up to 30 percent of the population in Europe. Apparently it has been shown that the odor receptors of human beings are particularly adapted to perceive human (body) odors. It would be interesting to see if the repertoire of basic odor terms in other areas of the world was similar to the list Hombert gives.

I'm happy to report that the paper I gave (a typological sketch a la Tom Payne of a Mande language called Jo) attracted the favorable attention of one of the editors of the journal *Mandenkan*, Denis Creissels, and will appear as a special number of that journal, probably in 1993. ■

REVIEWS OF BOOKS

Landscapes of emotion: Mapping three cultures of emotion in Indonesia. By Carl S. Heider Cambridge University Press, 1991. Hardback $44.50.

Reviewed by Genevieve M. Hibbs

As you have listened to someone describing some emotional situation have you ever been surprised at the seeming depth of the emotion in relation to its apparent cause, or the physical expression of the emotion or its complete lack? Read *Landscapes of emotion*, either by dipping into it, especially the second part, or by following the detailed methodology, and see the emotional expression differences displayed out before you. This can give unexpectedly new depths to understanding people, and of the kind of differences of emotional vocabulary and expression which can occur, even given the same individual when using different languages.

I found this book fascinating. Although the languages under study are very unlikely to be familiar to other than Minangkabau, Indonesian or Javanese people (and a few SIL members), the argument is in readable American English, and there are good links made to British English variations. Also, at all key points English glosses are given for easy understanding.

Any SIL translator could find this book helpful in trying to understand emotional connotations of language, and in applying the knowledge to particular language(s) in which they are working.

The extent of variation of use of vocabulary, and the relationships of behavior antecedent to and following experience is clearly displayed. For those learning about linguistics, dipping into sections of the book would be valuable in understanding more of the possibilities of linguo-ethnic variation, specifically as it effects the understanding and expression of emotion. One or two SIL members might even find themselves doing similar work on emotional vocabulary in the language(s) of people with whom they work. Heider's scholarly and thorough book gives enough detail of the methodology to make a start! ■

The Language of First-Order Logic, including the Macintosh program Tarski's World. By Jon Barwise and John Etchemendy. CSLI Lecture Notes, Number 23. Menlo Park, CA: Center for the Study of Language and Information, 1990. Pp. 259. $27.50

Reviewed by Thomas M. Tehan

Payap University (Chiang Mai, Thailand) and SIL Thailand.

Um, let's see. I think if I word it this way, 'If there exists a linguist who does not need to know some symbolic logic, then he or she should not read this book.' ... I think I can put that into symbolic logic. BUT it will definitely stretch my brain! Ah, but this is a book review (that will perhaps stretch your brain), so on with the task at hand.

Perhaps you have asked yourself the following question: 'As a linguist and/or translator, do I need to know symbolic logic?' Many of us have felt some inadequacy as we have read other scholars' articles that utilize logic symbols. Symbolic logic grammars and phonologies go through fads, but a basic knowledge of the possible and basic notations is becoming indispensable in our field. This book describes one very common representation of symbolic logic, or First-Order Logic (FOL); however, it also reviews other representations that are currently in use. If you understand this book, you can read symbolic logic.

FOL is not a full language. It's more like a pidgin; it's used by speakers of different languages (in different disciplines) to talk to each other about some things. Some people have acquired enough facility to be rated as good second language speakers, but there are no native speakers. Many others acquire a reading knowledge only.

So let's suppose that as a linguist, you have come to the conclusion that you could profit from some knowledge of FOL. This book can be a great aid to you, for each chapter you read brings a fuller understanding of the processes and notation of FOL. You may not want to remember all the details of the book's line upon line of symbolic geometry-style proofs (the bad news), although they do help to show whether you really understand. But wait, here's the good news—you can play an accompanying computer game to see if you really understand the concepts of each section, so you can skim a

little faster through some of the symbols of those drier proofs. By the way, the authors of this book do pretty well at making even these proofs readable for the average algebra-only reader.

For many linguists who want an introduction to symbolic logic, reading a chapter quickly, studying the summary boxes carefully, and playing the computer game called 'Tarski's World' will open a new area of understanding in symbolic notations. You don't have to remember all the terms (e.g. 'indiscernability of identicles', p. 24) in order to derive substantial benefit. After all, you are a learner, self-motivated, and not an enrolled student after some credit hours.

So what is this book? It is lecture notes for an introductory class in FOL, thus it contains a discussion of the key concepts of the symbolic logic. The purpose of the book is to acquaint the reader with the terms of FOL and to equip him to manipulate the language. Now let me first give a general description of the book, and then highlight some of the authors' comments on translation.

This book is one of the series of *Lecture Notes* from the Center for the Study of Language and Information (CSLI). It consists of an introduction and four parts:

1. Propositional Logic (3 chapters);
2. Quantifiers (3 chapters);
3. Applications of First-order Logic (2 chapters),
4. Advanced Topics (2 chapters).

The teacher in class or the individual reader can follow the authors' advice on sequencing. The material of the first two parts should be mastered and then as much of parts 3 and 4 as desired can be attempted. Parts 3 and 4 represent some of the various applications for first-order logic.

There are no illustrations, but truth tables (tables that are used to evaluate the truth value of each element in a symbolic statement and determine the whole statements' truth values), summary tables, and 'remember boxes' abound. The latter summarize the main points of a section and could be used for a review or a pre-test to see if the reader already knows the material in the section being summarized.

The first seven chapters (about one-half) of the book contain 'the basics'. After reading through these chapters and playing the game, the reader will have a grasp of the fundamentals of first-order logic. The linguist that wants to get a basic grasp of the topics (and this includes all of those who anticipate going back to school for another degree), will want to read through these.

Chapter 8 is a good review of set theory and its representation in FOL. Chapter 9 explains induction proof, consisting of a basis step and an inductive step. The discussion is a bit more mathematical in chapters 8 and 9, and there are no exercises using the computer game, Tarski's World.

Chapter 10 describes Horn sentences, a special type of symbolic logic statement that is used in computer science, in applications like Prolog. Automated theorem proving, which is one of the accepted approaches to artificial intelligence, is introduced. Chapter 11 "presents some more advanced topics in first–order logic... It is intended as a transition into more advanced logic courses of various sorts" (p. 209). The authors are more rigorous in their approach to some of the concepts introduced informally in earlier chapters. Tarski's game is back in chapters 10 and 11. (By the way, I never found out who Tarski is.) The number of 'remember boxes' decreases in the last four chapters.

Two appendices are included: A) How to use Tarski's World, and B) Macintosh terminology. A general index and a Macintosh index are also included.

Since this book is a textbook by primary design, there are many exercises. They come in two types. The first is the normal textbook exercise that presumably the student would complete and turn in to the instructor. The second type of exercise, to be done on the computer with immediate feedback in 'Tarski's World', is more fun.

Tarski's World represents a three dimensional space of relationships between objects that is displayed on the monitor screen. Sets of sentences can be compared to the various 'world set-ups' (located in files on the disk) based on the rules of first-order logic. The game is then played to determine whether what the player thinks about truth and falsity in the world is actually true. So a player can sharpen his or her logic processing ability at more and more complex levels.

It takes about a half-hour to get familiar enough with the game to begin doing the exercises fluidly. After that, exercises can be much more fun and profitable. It's more fun than exercises, not quite the educational game with scores and the exploding space ships of invading aliens, but it is certainly profitable in that the player can determine immediately at each discrete step if he or she is really understanding the material and able to do the problems.

There are a few very minor negative comments to make about the book. While there is a list of CSLI publications at the end, there is no general bibliography. A list of 'further reading' would have been helpful. Two pages (pp. 173, 213) contain a few suggestions of authors or books to pursue on some of the subjects, but these comments are rather incomplete. The book is very good at stimulating a desire to know more, but it seems the authors just expected us to take another course in logic to satisfy the appetite. Toward the end of the book, the authors further leave the reader with a taste for more by mentioning 'second-order logic' in a rather off-hand way.

On the whole, this book and the game are a whole lot more fun than my *Introduction to Symbolic Logic* course and easier to understand. The game gives feedback and practice. You know you understand the material, and you can enjoy gaining facility in manipulating the symbols. All of this makes it much better than the laborious and complex truth tables and proofs that I wrote out in class. If I were to teach a class in FOL, I'd choose this book as the course textbook. However, if I were to teach or to study the book on my own, it would be nice to have an answer guide to the problems. This could especially be helpful for the more advanced problems.

Notes On Translation

As a postscript, perhaps the readers of *Notes on Linguistics* would be interested in the authors' comments on translation, as they apply to FOL. Those of us who are new to symbolic logic may be impressed with its rigid definitions, symbols, and operations. We think, 'Symbolic logic is so tightly defined that translation involving it must be quite a bit simpler than dealing with the messiness of natural languages.' Perhaps this is so, but many of the same steps and cautions apply to translation from English (or any natural language) to FOL.

Barwise and Etchemendy write:

> How do we know if a translation is correct? Intuitively, a correct translation is a sentence with the same meaning as the one being translated. But what is the meaning? FOL finesses this question, settling for truth conditions. What we require of a correct translation in FOL is that it be true in the same circumstance as the original sentence... But there is a matter of style. Some good translations are better than others. You want sentences that are easy to understand... There are no hard and fast rules for determining which among several logically equivalent sentences is the best translation of a given sentence (p. 39).
>
> When an English sentence contains more than one quantified noun phrase, translating it can become quite confusing unless it is approached in a very systematic manner. It often helps to have a number of intermediate steps, where quantified noun phrases are treated one at a time (p. 118).
>
> Some English sentences do not easily lend themselves to direct translation using the step-by-step procedure. With such sentences, however, it is often quite easy to come up with an English paraphrase that is amenable to the procedure (p. 121).

One is reminded of exegeting and unskewing a text into concepts and propositions to facilitate translation as is often taught in SIL translation courses.

> There are a couple of things that make the task of translating between English and first-order logic difficult. One is the sparsity of primitive concepts in FOL. While this sparsity makes the language easy to learn, it also means that there are frequently no very natural ways of saying what you want to say. You have to try to find circumlocutions available with the resources at hand... The other thing that makes it difficult is that English is rife with ambiguities, whereas the expressions of first-order logic are unambiguous (at least if the predicates used are unambiguous). Thus, confronted with a sentence of English, we often have to choose one among many possible appropriate interpretations in deciding on an appropriate translation. Just which is appropriate depends on context (p. 124).
>
> The problems of translation are much more difficult when we look at extended discourse, where more than one sentence comes in (p. 125).
>
> There are many expressions of natural languages like English which go beyond first-order logic in various ways (p. 151).

The list of example problems should sound familiar to translators: 'if...then' does not always mean the same thing; the range and

number of determiners is not comparable, and some English determiners are not even expressible in FOL; singular and plural usage implies different things in each language; tense is problematic; many ideas captured by modal auxiliaries are inexpressible in FOL (pp. 151–152).

One final translation related topic may strike a familiar chord for those researching translation theory.

> The philosopher H. P. Grice developed a theory of what he calls conversational implicature to help sort out the genuine truth conditions of a sentence from other conclusions we may draw from its assertion. These other conclusions are what Grice called 'implicatures. ...Grice pointed out that if the conclusion is part of the meaning, then it cannot be 'cancelled' by some further elaboration by the speaker (p. 71).

For instance, 'Max and Claire are home' legitimately supports the conclusion that Max is at home. The cancellation 'but Max is not at home' cannot be appended to the statement without resulting in a contradiction. However, in the statement, 'You can have soup or salad', there is a strong but not absolute implicature that you cannot have both. Still, it is possible to say, 'You can have soup or salad ...or both' without directly contradicting yourself (p. 72). There's more on this to be found in the book.

Well, um, I think it would be appropriate to finish my first thought. It goes something like this:

$\exists x(\text{linguist}(x) \wedge \neg\text{need-to-know}(x,\text{FOL}) \rightarrow \neg\text{read}(x,\text{book}))$. ■

The acquisition of two languages from birth: A case study. By Annick De Houwer. Cambridge: Cambridge University Press. 1990. Pp. xv, 391. $49.50

Reviewed by Jim Lander

Those interested in language acquisition theory and who also appreciate a scholarly presentation are likely to enjoy *The acquisition of two languages from birth: A case study.* Hugo Baetens Beardsmore

advised Annick DeHouwer on this study. If his work interests you, this will as well.

The subject of the study is a little girl whose father is Dutch and mother is American. She is identified as Katie. Katie's father has spoken Dutch to her since birth—her mother has spoken English. Katie lived in Belgium from birth up through the time of the study, hearing both Dutch and English in the course of her normal day. The data records her language production over a period of eight months: from the seventh month of her second year to the fourth month of her third year.

The author, Annick De Houwer, began her work with a thorough review on methods and theories related to bilingual first language acquisition. Those new to the study of language acquisition will find reading the first two chapters to be profitable.

De Houwer went on to present her aims and hypothesis in chapter three. She focused on the morphosyntactic development of Katie's two languages. De Houwer's hypothesis sought to establish if, and to what extent, Katie's two languages developed separately. The researcher identified this as the separate development hypothesis. De Houwer was also concerned as to whether language acquisition is language dependent. That is, does the language being acquired somehow influence the manner in which acquisition proceeds? As in the earlier chapters, the discussion was complete, fully preparing the reader for De Houwer's summary of the data.

In chapter four the researcher described her study. Once again De Houwer was thorough, leaving no questions in this reviewer's mind as to how the study was conducted. The remaining chapters covered De Houwer's analysis of Katie's production of the noun and verb phrases, as well as her syntax, and metalinguistic behavior. De Houwer's syntactical analysis followed a coding system based largely on the European structuralist tradition. This, she noted, is the approach many researchers adopt.

De Houwer's approach was straight forward. For instance, in examining Katie's production of noun phrases, De Houwer first presented the data related to Katie's Dutch usage. The analyst then compared Katie's production of Dutch with what is known about the production of noun phrases by Dutch monolingual children of similar

age. She then analyzed Katie's English production of noun phrases and compared this with what is known about the production of noun phrases by English monolingual children of similar age. De Houwer also compared Katie's Dutch and English production of the noun phrase with each other in order to determine if either language was influencing the acquisition of the other. In this way De Houwer proceeded through each category.

Annick De Houwer concluded that her study of Katie's language acquisition affirms the separate development hypothesis and that language acquisition is language dependent. For example, Katie's acquisition of Dutch noun phrases proceeded in a manner different from her acquisition of English noun phrases. Neither language seemed to influence the acquisition of the other. DeHouwer's presentation is convincing. Those who disagree will have to contend with her data.

The author rightly pointed out that, because this study did not begin until Katie was nearly three years old, it can not affirm the independent development theory advocated by other researchers. The independent development theory proposes that in the bilingual acquisition of two languages from birth both languages develop independently of each other. De Houwer's data does not include Katie's early language production, so, in spite of the fact that Katie's two languages were independent during the period of the study, nothing is known about how the languages developed prior to the study. None-the-less, De Houwer's findings certainly do not contradict the independent development theory either.

The technical aspects of this book are remarkable. Relevant examples and tables are generously interspersed within the text. Appropriate end notes appear with each of the eleven chapters. An appendix of 36 more tables, 20 pages of references, and a seven page index of referents are also provided. A list of all the tables and a key for the symbols and abbreviations used in the text immediately follow the comprehensive table of contents in the front pages. The reader should have no difficulty in getting around in this book.

There was one quirk in this otherwise well laid out book. When the author cited non-English quotations, they were presented only in their untranslated form. That leaves the English reader in the dark, unless, of course, he, or she happens to know the language in which the

quote appears. In fairness, quotations such as these are relatively few. So, overall comprehension of the material does not suffer significantly.

In *The acquisition of two languages from birth: A case study*, Annick De Houwer provides a scholarly and thorough study of bilingual first language acquisition. The editors of the *Cambridge Studies in Linguistics* series thought enough of her work to include it as a supplementary volume. This reviewer hopes her contribution receives the attention it deserves. ■

Referential Practice: Language and Lived Space among the Maya. By William F. Hanks. Chicago: University of Chicago Press, 1990. xxii, pp. 572. Hardback $65.00, Paperback $27.50

Reviewed by James K. Watters

Many of us have heard Pike's admonitions regarding the analysis of linguistic (and other human) behavior:

> In my view, reality-as-knowable-by-man lies not in abstraction-from-man but in person-in-relation-to-item. Man cannot know a certain thing without having some kind of relation, direct or indirect, to that thing... Abstraction totally away from all observers (including the native speaker and including actions of inference or imagination by them) and claiming for that abstraction an ultimate reality would be a distortion of the reality of person-in-relation-to-item being implicitly studied (even when denied) by an analyst (1981:85,6).

We agree but have a hard time knowing how to do our field work and present the results in a way that demonstrates such convictions. Hanks has provided an example of such work in this detailed study of one aspect of linguistic structure and language use.

This is a book on deictic constructions,[1] specifically those found in (Yucatec) Maya. But it is much more than that. Hanks provides a new framework for dealing with how people use linguistic forms, not only to refer to objects in the world around them, but to construct the dimensions of their social and cultural space.

As Hanks points out, traditional accounts of deixis, helpful as they are, typically treat it in the abstract, as a system of spatial and temporal relations centered on some point. The employment of the system in actual situated speech events is assumed to involve simply the anchoring of that central point to the speaker at the place and time of the speech event. The forms are pointers, directly designating objects or times in the world from an egocentric (speaker-centered) base.

The resulting picture of deixis is a simple one: what is usually lacking is a set of detailed accounts that confirm the analysis by showing how the forms are used in attested everyday conversations. Any attempt to do so in a serious manner would presumably result in a hopeless multiplication of features, trying to account for the wide variety of uses across situations. Such an account would no doubt provide interesting anecdotes but be lacking in generalizations. So the most we expect is a paradigmatic listing of proximate vs. distal forms, pronouns, etc., and observations on their most typical uses. To go beyond that in any significant way would be a formidable challenge.

Hanks takes on the challenge. His concern is 'with describing the material specificities of things said and places occupied by real people on actual occasions in the community of Oxkutzcab, Yucatán' (p. 517). Yet, instead of getting lost in the details, he presents significant generalizations regarding the meaning and use of the deictic constructions.

The key, in part, is realizing that deixis does not involve simple, direct specification of objects in the world. The relations between the speaker and interlocutor, their bodily space, their location, and the linguistic categories they have at their disposal, are all based on a background of social and cultural knowledge. This knowledge is structured and includes schematizations of their experience in the world. The relation between the actors and the objects of reference, then, is not simple and direct; it is mediated by this background knowledge with schematic structures developed from previous interactions. Hanks, following both Fillmore's work in semantics and Goffman's work in the sociology of interaction, uses the term frames to refer to these schematic structures.[2] Frames are part of the knowledge brought to the interaction by the participants and impose 'emic' structure on the 'etics' of the situation.

Distinguished from the frame is the framework and frame space (also from Goffman). The former is the transient arrangement of the actual speech event within which frames are applied; the latter is the set of frameworks that are possible at the moment of the speech event—those that are unrealized as well as the one currently in effect.

The introduction of these and other categories of analysis from the literatures of linguistics, sociology, phenomenology, and anthropology, along with concepts peculiar to Hanks is unquestionably demanding on the reader. However, this is just the sort of thing we expect to come up against reading a book that explores new territory. The fact that the book has excellent name and subject indexes lessens the difficulty.

Hanks presents deictics as having a relational value which serves as a function from the indexical ground to the referential object. From the base of the activated frame the interlocutors locate the object by means of the relational value of the deictic element. Significantly, the indexical ground includes a sociocentric rather than an egocentric framework. Deixis is not centered on the individual speaker, as in traditional accounts, but on the social interaction of the participants.

As Hanks demonstrates, the frames associated with the Maya deictics are not just part of an isolated subsystem but apply across domains of experience, i.e. within a wide variety of frames: corporeal, residential, agricultural, ritual, etc. This is one of the truly impressive feats of the book: Hanks shows that 'the frames instantiated in spatial deixis are identical to, or homologous with' frames associated with other socio-cultural domains that he sketches in the book (p. 297).

The book is divided into four main sections.

Part I

Part I, 'Social foundations of reference' includes the first three chapters. Here Hanks introduces the basic terminology used throughout the book as well as a paradigmatic sketch of Maya deixis. These constructions for the most part consist of an initial deictic element (ID) occurring in phrase- or sentence-initial position and a terminal deictic element (TD) occurring in phrase- or sentence-final position. (In some constructions the ID and TD are juxtaposed.) As is shown throughout the book, the ID forms specify the grammatical

category of the element, e.g. noun or pronoun, sentential adverb, circumstantial adverb. Each of these grammatical categories, in turn, have a primary association with one dimension of deixis, what Hanks calls the 'core dimension' of the category, e.g. 'participation' (in the speech event, i.e. first, second, third person), 'perception', 'space' (location of object).[3] The TDs specify values within the dimension, the 'relational values' mentioned above.

Also in this section, in chapters 2 and 3, Hanks argues for a sociocentric rather than egocentric account of deixis, elaborates on frames and frameworks, encoded vs. conveyed meaning, and schematic knowledge (brought to the interaction by the participant) vs. local knowledge (a product of the interaction). He discusses the 'embodiment' of language (following work in philosophy by Merleau-Ponty and, more recently, Mark Johnson), especially relating to spatial effects of body orientation and the frames associated with the typical Maya residential unit. We are also presented with detailed accounts of the situated use of deictic forms (in digging ditches), demonstrating that 'asymmetries of indexical reference also connect with asymmetries of social status' (p. 125); e.g. the use of forms similar to 'here' and 'there' are in part determined by who is boss.

Part 2

In the second section (chapters 4, 5, and 6) Hanks addresses particular portions of Maya deixis: personal pronouns and what he calls 'ostensive evidentials'. The former are introduced in a rather heavy discussion (bordering on pedantic) followed by an insightful discussion of complex participation frames. These are given graphic representation, illustrating embeddings and transpositions of the frames. The ostensive evidentials include deictic forms that function as presentatives, directives (directing the addressee's attention to some object), and for marking both type of perception (tactual, visual, discourse) and certainty of perception.

Part 3

The third section, on 'Space and spatial reference', presents a basic Maya schematization of spatial orientation: the grid of the compass reckoned relative to a center (e.g. the activity or speech event). In some activities the relevant features are the cardinal points, in others,

the cardinal directions. The former refer to locations and are especially typical of shamanistic ritual. Such activity can create a space, defining a residence or field by (verbally) drawing the lines between the four points and so setting its perimeter and making it safe.

The cardinal directions are typical of everyday conversation, defined not by the perimeter, but by lines of orientation running through the center—the ground of reference by which the referent's location is specified. This basic schema is shown to operate at all levels of Maya society, defining orientations within the altar, the household, the field, and the cosmos.

The final chapter of the section, chapter 9, discusses spatial frames relevant to the deictic nominal constructions. Again Hanks demonstrates that, contrary to traditional accounts of deixis, the attribute 'egocentric' is simply one parameter within the Maya system and not the defining feature. Throughout the section Hanks again highlights the nonabsolute nature of the indexical ground. It can be contracted or expanded ('here on the page' vs. 'here in Mexico') and it can be embedded or transposed.

Part 4

The fourth and final section, 'Structure in referential practice', discusses the features of Maya deixis that allow it to be characterized as a system (chapter 10) and the wider implications of the study (chapter 11). Maya deixis is a system made up of subsystems of parallel structure and marked by distinct forms that are found across the various subsystems (the terminal deictics or TDs). Hanks moves on here to try to account for why Maya deixis is structured as it is (but does so rather unsuccessfully, I believe—and as I would expect, considering the nature of the question).

The wider implications to which Hanks draws our attention primarily deal with the two features of traditional accounts of deixis that have rendered them overly simplistic: an unwarranted objectivism and a degree of abstractness 'that has helped to shield theory from the real complexities of actual language use' (p. 517).

Those linguists that are unsatisfied with what counts as explanation in the anthropological literature will no doubt find the same discomfort

in reading the accounts in this book of why speaker X used such-and-such a form on a particular occasion. But this will be true of any account that moves beyond abstract linguistic structure to actual language use. Chomsky, distinguishing the study of linguistic and other cognitive structures from that of human behavior, has articulated this unease in the following way:

> When we ask how humans make use of these cognitive structures, how and why they make choices and behave as they do, although there is much that we can say as human beings with intuition and insight, there is little, I believe, that we can say as scientists (1975:138).

The difficulty has to do with the notion prevalent within the generative tradition of science-as-prediction.[4] Such a view of explanation is distinct from that found in most of the anthropological tradition:

> A characteristic of scientific explanation is that it allows predictions, since it attempts to supply the causal factors behind a phenomenon so that when the appropriate conditions exist the phenomenon can be expected. By contrast, meaningful [anthropological] explanation attempts to make a phenomenon intelligible, and the issue of prediction does not arise (Hatch 1973:336).

In recent years a number of linguists have reacted against the (formal) generative tradition in linguistics in part because prediction is usually an unreasonable goal when dealing with pragmatics, discourse, and lexical semantics. After presenting an account of the semantics of a Japanese classifier Lakoff states:

> The traditional generative view that everything must be either predictable or arbitrary is inadequate here. There is a third choice: motivation (1987:107).

This study by Hanks provides an outstanding example of how one can present convincing explanations of language use, motivating the account from within a richly developed theory of social interaction and spatial orientation.

Notes

[1] These are often described as linguistic forms whose interpretation is directly grounded in the speech event ('I', 'you', 'here', 'there', 'then', 'now', etc.). Thus Anderson and Keenan describe deictics as 'those linguistic elements whose interpretation in simple sentences makes essential

reference to properties of the extralinguistic context of the utterance in which they occur' (1985:259).

[2] It should be noted that Fillmore uses 'frame' to refer to a set of lexical items associated with concepts of a particular realm of experience such as 'days of the week' or 'commercial event'. Goffman uses it to refer to a set of alignments or relations among the interlocutors, the speech, and the context of an interaction. However, when Hanks accounts for how deictic forms are used in actual conversations both senses appropriately come into play.

[3] Actually, Hanks claims the IDs *encode* both the grammatical features as well as metapragmatic features (by specifying the core dimension). It would seem more accurate to say they encode grammatical category and convey the metapragmatic features (due to the fact that the latter are associated with that category).

[4] What relation such a view might have to an obsolete falsificationist view of science is better left to others to determine.

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Conditions on phonological government. 1991. By Monik Charette. Cambridge: Cambridge University Press, Cambridge Studies in Linguistics 58. $49.95.

Reviewed by Mike Maxwell

This book, a revision of the author's Ph.D. thesis, presents a theory of phonology known as phonological government, and uses that theory to describe a well-studied process of deletion (or epenthesis) of schwa in

French. The author's purpose '...is not to provide yet another account of the behavior of schwa, but rather to use the French facts to develop...the theory of phonological government' (page 214, note 1).

The process in question is an alternation exhibited by many words of French between a form which contains a schwa vowel ([ə]) and a form in which that vowel is absent. (The vowel in question is traditionally known as e-muet or unstable-e.) For example: *petit gars* [ptigɑ] young boy, *petitesse* [pətitɛse] smallness. Oversimplifying, the environment in which the schwa is omitted is word finally or word-internally preceded by a vowel plus a single consonant, or in the first syllable of a bisyllabic word when preceded by a single consonant. There are, however, numerous exceptions, some phonologically conditioned, but some which appear on the surface not to have any phonological conditioning. Chief among the latter are words containing an h-aspiré, a term which refers to something which in many ways behaves as if it were a consonant, but is not pronounced. (It is written with the letter *h* in the French orthography.) To further cloud the picture, there are words in which schwa alternates with [ɛ]. In short, the situation is complex.

The Theory of Phonological Government

Faced with an alternation between a vowel and the absence of that vowel, the first question most linguists would ask is whether the alternation is an instance of epenthesis or of deletion. Charette argues instead that neither epenthesis nor deletion is the answer: the underlying (lexical) form of words which undergo this alternation contains an empty vowel, which is either assigned phonetic form by general principles, or remains null (unpronounced) at the surface (phonetic) level.

Charette's analysis may be broken down into three components, which are independent in the sense that one or two might turn out to be false without invalidating the others. These components are: the hypothesis of an underlying empty vowel slot; the principles by which this empty slot is or is not allowed to remain null; and the principles by which the empty vowel is assigned a specific phonetic form. I shall have the least to say about the last part, which is intended in part to explain why similar vowel alternations affect different vowels in other languages. Based on the phonological theory of charm and

government (see Kaye, Lowenstamm and Vergnaud 1985, and Coleman 1990 for some discussion), it is perhaps the most dispensable of the three components of Charette's theory, in the sense that her theory would still make interesting predictions without it.

The notion of an underlying empty vowel slot, on the other hand, is a very interesting concept, one which might have implications for quite different sorts of phenomena than the French case.[1] Going beyond the question of the French data, Charette (following Kaye 1990) proposes that all words in all languages have a word-final vowel, which may be underlyingly empty in certain languages. Since empty vowels may be realized as null provided the appropriate conditions (outlined below) are met, the apparent existence of word-final consonants in many languages may be accounted for.

Charette motivates this seemingly preposterous proposal by her analysis of French (see also Kaye 1990 for a less detailed discussion of a wider range of languages). Since her argumentation is too complex to lay out in this review, I will instead sketch a reanalysis of a traditional problem in English morphology. The problem is this: Is there a principled way of determining the underlying form of the plural noun suffix (etc.) in English? Is it the form with a vowel in words like *bushes* and *bases*, while words like *hats* and *dogs* are derived by a rule of vowel deletion? Or does the underlying form lack a vowel, while the form in *bushes* is derived by a rule of vowel epenthesis? Charette's answer would (presumably) be that the underlying form of the suffix lacks a vowel,[2] but that the vowel that appears in words like bushes is not only not epenthetic, it is part of the stem, novel solution, to say the least.

Given that all words must end in vowels, Charette erects a typology of languages based on two parameters, which the child learning a given language must set. The first is whether the language has underlying word-final empty vowels. The second (relevant only if the answer to the first is yes) is whether the underlying vowels may remain empty at the surface. (A third parameter accounts for the difference between languages which allow word-final consonant clusters versus those which allow only a single consonant word-final.[3]) Unfortunately, she doesn't clarify the distinction between languages which disallow underlying word-final empty nuclei, and languages which allow such underlying nuclei, but prohibit them from being manifested as null at the surface. (Possibly the distinction is intended to correlate with the

existence of word-internal closed syllables in a language, but this is not made clear in Charette's discussion.)

Returning to French, Charette argues that the appearance of word-internal and word-final empty nuclei at the surface (phonetic level) is predictable on the basis of several constraints. When the constraints are satisfied, an empty nucleus may remain null at the surface; otherwise it must be given phonetic form, thus accounting for the alternation between a schwa and zero. The two main constraints that an empty nucleus must meet in order to remain null at the surface, are that it be either properly governed (which affects word-internal empty nuclei) or licensed (which affects word-final nuclei). The balance of the book describes how these constraints explain the French alternation between schwa and zero.

The names of the constraints and the relations which determine their application are chosen to parallel the nomenclature of the syntactic theory of Government and Binding, emphasizing the presumed parallel between empty categories in syntax and empty segments in phonology. In my opinion, this terminology is at best confusing and at worst misleading. Consider, for instance, the terms *government* and *proper government*. In syntax, a head governs all its complements, and whether these complements are to the right or the left of the head is a language-specific parameter. In the theory of phonological government, a relation of constituent government holds between the left-most segment of some constituent (i.e., an onset, nucleus, or rhyme[4]) and its sister constituent to the right only; while a relation of interconstituent government holds between two segments which are not sisters, with the governor being adjacent and to the right of the governee. But there is no syntactic equivalent to interconstituent government, nor is it clear in Charette's theory that interconstituent government is really the same sort of notion as constituent government.

As for proper government, in Charette's theory a (syllable) nucleus properly governs another nucleus if it governs that nucleus and it is not itself governed.[5] A properly governed nucleus may then be realized as null. While the first half of this definition is comparable to the definition of proper government in syntax, the second half is not.[6] What the second half is intended to do is to ensure that in a sequence of two empty nuclei, not separated by another nucleus, one or the other of the empty nuclei will be phonetically realized

(non-null). But in order for this to work, it must be possible for the nucleus which is to be phonetically realized to somehow refuse government. (An ungoverned empty nucleus must be phonetically realized.) The idea that a constituent may remain ungoverned despite its appearance in a governing environment is completely foreign to syntax.

To say that the terminology of government phonology is confusing or misleading is not to say that the theory is necessarily wrong. If the putative parallel with syntax is ignored, it might be possible to recast the theory in terms having to do with syllabification instead. In the case of proper government, one might argue that in a sequence of two empty nuclei, it cannot be the case that both nuclei remain null at the surface because it would be impossible to form a constituent containing both the empty nuclei together with some non-empty nucleus. Alternatively, one might place the blame on the failure of the intervening consonants to syllabify, although that would be getting further from the spirit of Charette's theory. Whether the entire theory could be recast in phonological terms without losing its essence is another question, but it might be more plausible, and it would certainly be more intuitive. As it stands, the argumentation is difficult to follow in part because of the forced usage of syntactic terminology.

Turning to broader difficulties, Charette's study is concerned chiefly with vowel-zero alternations of the type found in French, in which the disappearing vowel always has the same phonetic shape in its non-null form (or phonetically conditioned variants, in this case [ɛ] and [ɔ]). It is difficult to see how it could be extended to vowel-zero alternations in languages (such as the Jivaroan languages) in which a vowel of any phonetic shape can be omitted. This may not be a difficulty, however, if it can be demonstrated that such alternations have properties which differ from those of vowel-zero alternations of the French type.

It is also unclear whether consonant-zero alternations can be given a similar analysis. Charette discusses several cases of consonant-zero alternations, but her account of them is quite different from her account of vowel-zero alternations: an unlicensed (and ungoverned) empty syllable nucleus (vowel) must be phonetically realized, whereas an unlicensed consonant must be phonetically **un**realized. While this seems contradictory, it is perhaps better seen as another confusion

brought on by the terminology. After all, one would expect syllabic and non-syllabic segments to behave differently, if such alternations are driven by syllabification.

Finally, one wonders whether the phonological government theory, with its strict limits on possible syllable structure, would be too procrustean a framework for languages with more complex syllable structures—particularly for languages (such as the Salishan languages of the Pacific Northwest of North America) with such long consonant clusters that even the existence of syllables has been doubted.

Other Remarks

This book would have greatly benefited from the efforts of a good editor. The style is highly repetitive, for example:

> Because no vowel follows it, this final empty nucleus is not properly governed and so must have a phonetic interpretation. The vowel [ö] is realized. Because it is not properly governed, the word-final nucleus receives a phonetic interpretation [page 70].

In addition, such typographic niceties as the use of bolding and the placement of footnote numbers are used inconsistently, causing confusion on the part of the reader, particularly in some of the diagrams depicting syllable structure.

Typographic errors and the like are few. Among those which might cause confusion, I noted the following: footnote 5 on page 210 refers to Lowenstamm (to appear), but this work is not listed in the references; footnote 20 on page 211 refers to Harris 1990, but in the bibliography there are two references to works by Harris with this date—it is not clear which (or both) is intended. On page 73, example (6) depicts the marked value of the feature α^0, but the text immediately above this example refers to the feature α^+; apparently the text is correct. On page 108, lines seven and eight read '...except in syllable-initial position...' but should read '...except in word-initial syllables...' Finally, on page 124, line four reads '...when the preceding cluster is...' but should read '...when the following cluster is...'

Implications

Perhaps the chief use of a study like this is to point out the depth of knowledge of a language needed to decide such an apparently simple question as epenthesis versus deletion. What looks like a simple problem at the beginning of the book is shown to be fraught with complications. If you as a field linguist are faced with a question of vowel-zero alternations, it may pay you to read this book as a source of suggestions for data to look for. But be forewarned: it is not easy going, and a preparatory study of recent theories of phonology is to be recommended. Specifically, Goldsmith (1990) will provide a good overview of recent approaches to generative phonology, albeit from a different theoretical perspective. Then read Kaye (1990) for background on Charette's particular approach.

Notes

1 The idea is not original with Charette; Anderson proposed it earlier for this same French alternation.

2 More precisely, the suffix begins with a consonant. It will presumably have a final empty vowel (which in this case never surfaces).

3 I note in passing that with the introduction of the notion of parametric variation, typologies are again fashionable in generative circles.

4 Government phonology is in the odd position of recognizing the traditional syllable constituents of onset, nucleus and rhyme, but neither syllable codas (= the rhyme less the nucleus) nor syllables.

5 Syllable nuclei are held to be adjacent at a certain level, despite the fact that consonants may intervene.

6 The notion of proper government in syntax has undergone several changes, but the following definition (from Chomsky 1981, page 273) is perhaps the closest to Charette's notion: *properly governs if and only if governs and is lexical.*

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ERRATA

I. Eddie Arthur in Côte d' Ivoire indicated that two errors were made in his article, *Speech-led versus comprehension-led language learning*, in NL 60. The following underlined highlight the corrections. On **page 22** in the second italicized paragraph, the third line should read: *...the new language and is not made to produce it...* On **page 32**, in the ninth line, the first name of Ms. Oxford was incorrectly printed and should read Rebecca Oxford. We extend an apology to her for this error.

II. Usually we have pretty good luck in translating RTF files, but we managed to mangle Mike Maxwell's review of *Conditions on phonological government* by Monik Charette. Underlined are the needed corrections to his review in NL 61.

Page 59. 2nd paragraph: The process in question is an alternation exhibited by many words of French between a form which contains a schwa vowel ([ɔ]) and a form in which that vowel is absent. (The vowel in question is traditionally known as e-muet or unstable-e.) For example: *petit gars* [ptigɑ] 'young boy', *petitesse* [pɔtitɛse] 'smallness'.

Page 60. Last sentence of 3rd paragraph: Charette's answer would (presumably) be that the underlying form of the suffix lacks a vowel,[2] but that the vowel that appears in words like bushes is not only not epenthetic, it is part of the stem—a novel solution, to say the least.

Page 63. Extended quote in middle of page, second sentence: The vowel [i] is realized. (Last paragraph, 3rd sentence) On page 73, example (6) depicts the marked value of the feature A^0, but the text immediately above this example refers to the feature A^+; apparently the text is correct.

Page 64. Footnote 6: The notion of proper government in syntax has undergone several changes, but the following definition (from Chomsky 1981, page 273) is perhaps the closest to Charette's notion:

α properly governs β if and only if α governs β and α is lexical.

■

Coordinator's Corner

One of the hopes we have had for NL is to stimulate readers along both practical and theoretical lines, so when we get feedback indicating that specific articles have been successful, we are gratified. In this issue we have an instance: The Letters to the Editor column contains a response from Dwight Jewett to Howard Law's article on Focus Shift, in which Dwight shows how he responded to and advanced on Howard's ideas. Howard, in his original article, treated an interesting phenomenon as far as his data permitted. He then drew some projections and raised a number of questions.

We might wonder how he would respond to Dwight's exceptions. I hope you don't fail to read Howard's warm words which we publish preceding the letter from Dwight. Howard's response is a model for any scholar who makes claims with the intention that others test them.

The articles by Verhaar, Ndimele, Will and Weber show how important it is to be reading current linguistic literature if one is to make an interesting observation about some aspect of the language under study. I think all four authors show that if you don't know what the theoreticians' claims are, you are handicapped in framing your own findings. That is one reason for doing (and reading) reviews. Some articles or reviews might give you the feeling of being glad that you aren't in the theoretical camp of the author; others might make you wish you were.

This issue, Number 62 of NL, is my final opportunity as SIL Linguistics Coordinator and Editor of NL, to express my appreciation for the help of Betty Philpott, our format editor. She already has Number 63 (October '93) formatted. For the next issue we welcome Dr. David Payne, the new Linguistics Coordinator and new Editor of Notes on Linguistics. David hails from Tyler, Texas, got his Ph.D. in UT Austin, has had a wealth of experience in Campa (Arawakan) languages in Peru, and is also still director of the summer SIL course in Oregon. His skill and experience will be most welcome.

— Eugene Loos

Letters to the Editor

Who's Talking to Whom?

The following letter from Dwight Jewett is a welcome extension to the considerations of my article, 'Focus Shift Problem', *Notes on Linguistics* No. 57, May 1992, pp. 31-36. It is being printed in its entirety and treated as an article, although longer than normally allowed, because of its relation to the original article with its many aspects and legitimate questions. It specifically offers counter evidence to my conclusion expressed on p. 35 that 'shift of focus occurs only in poetical works and passages'. It also answers my questions regarding its occurrence in the New Testament, in Greek, and in other languages.

Shift of focus occurrences in translation as a problem was raised and is expertly addressed by Jewett, although some problems are left unsolved. These problems and additional questions raised by Jewett need to be addressed. For example:

What other languages exhibit shift of focus features?

How is it handled in these languages? In what languages could it be discovered if the linguist looked for it?

Is Jewett's treatment of 'grounds' relevant in other instances and languages?

How does Longacre's 'mitigation' in First John (*START*, No. 9, Dec. 1983) relate to shift of focus?

Are there other figures of speech or stylistic devices similar in form or function to shift of focus like Ixil's 'backwards word' that need attention by the translator?

What socio-linguistic features of communication need to be dealt with for linguistic functions like that of shift of focus, mitigation, and 'backwards word'?

What misunderstanding or lack of understanding is present in existing translations because no attention has been given to the figures of speech and stylistic devices? Is context being relied on too heavily to communicate the correct details of the message?

As we said at the end of the 'Focus' article, 'The editor and this writer would welcome your response and suggested solutions to this problem.' (And now, these problems!)

s/ Howard W. Law, Ph.D.

Dear Editors:

Thank you very much for the extremely interesting article entitled 'Focus Shift Problem' in *Notes on Linguistics* No. 57. The same evening I read that article we encountered that very phenomenon in our family Bible reading time. I was reading aloud from I Samuel 20. As we reached verse 15, we read these words which Jonathan spoke to David: '...and do not ever cut off your kindness from my family—not even when the Lord has cut off every one of **David's** [i.e. not **your**] enemies from the face of the earth' [NIV]. My two daughters (ages 10 and 13) exclaimed almost simultaneously, 'Now wait a minute. Who's talking to who? (sic) That doesn't make sense.'

The problem, of course, is that all through verses 12 to 15 Jonathan is speaking face-to-face with David. Yet in verse 15 he inexplicably switches to third person after having addressed David exclusively in second person in verses 12-14. I cannot explain the focus shift in this verse, nor can I account for the fact that the NIV differs from the NASV in verse 12 where the latter has: 'When I have sounded out my father... if there is good feeling toward **David**...' The NIV, on the other hand, says: 'I will surely sound out my father... If he is favorably disposed toward **you**...' It's a mystery to my why the NIV made the adjustment to second person in verse 12 but not in verse 15.

I cite this specific example to point out two things:

1) focus shift **can** cause the hearer/reader difficulties in keeping the participant reference straight throughout a discourse, as evidenced by my daughters' reactions;

2) this passage, which is not poetry, is a counter-example to the tentative conclusion stated in the article, namely that '...shift of focus occurs only in poetical works and passages.'

It is true that the focus shift here is not of the same magnitude as that found in Hebrew poetry, where it involves whole stanzas. Nonetheless, it is still focus shift as you define it.

Until reading your article I was not aware that this device was, in fact, a figure of speech in Hebrew nor that it had a name. Nonetheless, I have been intrigued by this phenomenon for some time. I have for several months kept an informal record of the instances I have encountered during my devotional reading through parts of the NIV Old Testament. As it turns out, it seems every instance I jotted down is functioning in a similar way. I will call this function 'deference to royalty'. It seems this is probably the most common usage of this device in the OT. My list of its occurrences contains Daniel 2:11, 36, 45b, as well as Esther 1:19-20; 2:3; 3:8-9; 5:4,8; and 8:5. I'm sure there are many others. (Two asides to note here: 1) I have not checked these occurrences of the phenomenon in the NIV against either the NASV or the Hebrew text; 2) I am not sure whether the 'deference to royalty' function accounts for the occurrence in I Sam. 20:15 which I cited above. It is true that Samuel had already anointed David king in chapter 16, even though he was not yet recognized as king.) In short, the OT (or at least certain parts of it) seems to be replete with this usage of focus shift.

On a related topic, I have a growing suspicion that this phenomenon is not confined to the OT. I further suspect that our understanding—or lack of same—of focus shift in certain passages in the New Testament has implications for both exegesis and translation. Specifically, I have been focusing on the switch from second person to third person in various parts of the pastoral epistles. My investigation so far leads me to the tentative conclusion that Koine Greek also employs a kind of focus shift. However, I believe the function of the phenomenon in Greek is quite different from its function(s) in Hebrew. In some of Paul's pastoral epistles, for example, I believe the switch from second person imperative forms to third person forms is another example of focus shift which has a different function from that of the corresponding device in Hebrew.

Consider II Timothy 2:3, where Paul admonishes Timothy in second person, i.e. [You] 'Endure hardship with us like a good soldier of Christ Jesus.' Then beginning in verse 4 he states the grounds for this exhortation by referring to three different kinds of people. But interestingly the grounds are stated in third person! '**No one** serving as a soldier...**he** wants to please **his** commanding officer...; **an athlete**...; **the hard working farmer**...' Obviously Paul intends that Timothy identify himself in some way with these people. In other

words, these third person examples are intended to have an impact on Timothy (second person in the communication situation). Paul is clearly using a series of metaphors in third person to drive his point home to Timothy. Why didn't he 'make the appropriate adjustment' by using a simile to spell out the point(s) of comparison explicitly, i.e. you Timothy are / should be like...? Perhaps focus shift is inherent in any metaphor used in an 'I-thou' communication situation. Or could it be that focus shift in such a communication situation in Koine Greek serves to make an exhortation apply even more directly to the one to whom it is aimed by emphasizing the grounds for that exhortation in such a way as to drive it home with an added punch? More likely, it seems to me, what we have here is an example of what Longacre referred to as 'mitigation' in his article *Exhortation and mitigation in First John* in START, No. 9 (Dec. 1983).

Again, a similar usage of third person occurs in II Tim. 2:21: 'If a man cleanses himself from the latter, he will be an instrument for noble purposes...' As in the above example, I believe this is grounds for a couple of exhortations, this time those in 2:15 and 16. Again, this looks like a mitigated imperative to me. Then one more analogous focus shift takes place in the same chapter. In 2:24-25, Timothy is obviously the one being referred to when Paul says: '**The Lord's servant** must not quarrel; instead, **he** must be kind to everyone... Those who oppose **him he** must gently instruct...' Again these words serve as an extended grounds for the admonition in the preceding verse: [You] 'Don't have anything to do with foolish and stupid arguments, because you know they produce quarrels.' Notice that there is no metaphor involved this time. From these passages it seems fairly clear to me that focus shift is a device used by Koine Greek to mitigate certain second person imperatives.

What isn't nearly so clear to me is what is going on in II Timothy 1:12b. After translating I Timothy, I began to do a draft of II Timothy with my co-translator. When we came to 1:12b, my immediate reaction was that Paul was still addressing Timothy (second person in the communication situation) when he wrote these words in third person (again quoting from the NIV): '...because I know whom I have believed, and am convinced that he is able to guard what I have entrusted to him for that day.' That is, I had a feeling 'deep down in my bones' that Paul way saying to Timothy: I know you well, Timothy, and that you are able to carry on faithfully

the ministry of the sound doctrine—which I am entrusting to you—until the day [when Jesus comes again].

This interpretation, I believe, is totally consistent with a major thrust of Paul's first letter to Timothy and certainly with the rest of the second epistle also, i.e. that Paul, the aging apostle now in prison, is entrusting the ministry of the sound doctrine to Timothy, who seems for some reason (incipient persecution of believers?) to be a bit hesitant to discharge the responsibility Paul is laying on him. Consider this verse (II Tim. 1:12b) in the light of the following passages: I Tim. 1:18,19; 4:14; and especially 6:20; II Tim. 2:3; 4:1,2,5; and again especially 1:6-8. In this latter passage, notice specifically the admonition to '...not be...ashamed of me his prisoner. But join with me in suffering for the gospel...' (v. 8b) and then the return to the same theme in v. 12: 'That is why I am suffering as I am. Yet I am not ashamed, because I know whom I have believed, and am convinced that he is able to guard what I have entrusted to him for that day.'

I am aware that 1:12b is a bit of an exegetical nightmare, given that the relative pronoun translated **whom** above, i.e. 'I know what I have believed...' would also be a legitimate rendering of this clause. To add to the complexity, 'what I have entrusted to him' is literally 'my deposit'. In discussing this verse, commentators seem to universally take **him** to be a reference either to Christ or God. Most prefer Christ because He is mentioned in the preceding few verses.

The question that I have is this: Considering the fact that focus shift elsewhere in the same epistle clearly serves to convey the grounds for an exhortation, why has not more thought been given to the possibility that 1:12b is serving as ground for Paul's exhortations to Timothy in 1:8, i.e. '...do not be ashamed to testify about our Lord, or ashamed of me his prisoner. But join with me in suffering for the gospel...' Granted there is quite a bit of material between the exhortations and the supporting grounds. But the following verses (1:13-15a) seem to me to be at least strongly parallel and probably chiastically structured (another Hebrew/Greek literary device) when considered alongside verse 12b. This causes me to believe that **whom** is probably the correct rendering in 1:12b and that it refers to Timothy. Look at the parallels in the following display adapted from Marshall's (1958: *The R.S.V. Interlinear Greek-English New Testament*)

literal interlinear gloss line (since I haven't taught my computer to speak Greek yet):

A I.know...whom I.have.believed [=whom I have trusted] and I.have.been.persuaded that able he.is [=you are?]

B The deposit of.me to.guard to that — day

C a.pattern Have.thou of being.healthy words which from me thou.heardest in faith and love — in Christ Jesus;

B' the good deposit guard through Spirit [the].Holy — indwelling in us

A' Thou.knowest this, that turned.away.from me all the.ones in — Asia

That's enough speculation about the exegesis of that verse. It's fascinating to think about what role focus shift might be playing there.

In closing I want to return to what I said earlier about our understanding, or lack thereof, of focus shift having implications for translation. Ixil, the lovely Mayan language I've been trying to get into my head for several years, almost never uses focus shift. However, the one usage I am aware of is in what the Ixil people call (literally) a 'backwards word'. In such a situation the speaker uses a rather indefinite third person form of speech to talk **to** another person in the communication situation **about** someone else who is also present. Invariably what is said is of a negative or critical nature and is intended as an insult to the one listening in, i.e. not to the one to whom it is directly spoken.

If my analysis of both Paul's usage of focus shift in II Timothy and the Ixil usage of the device is correct, a translation which does not make the appropriate adjustments in person could theoretically leave the Ixil reader with the impression that Paul was indirectly criticizing Timothy rather severely. This in spite of the fact that there seems to be no one else in the communication situation to whom Paul would be directly addressing his comments. Unless, of course, his amanuensis?! I think there is enough information to the contrary built into the context to prevent an Ixil reader from coming to this conclusion, but it is at least a theoretical possibility.

Yours for getting our focus properly shifted,
s/ Dwight Jewett, CAB - Guatemala

Head Shift: A Diary Entry

John Verhaar

Indonesian Linguistics Development Project, University of Leiden

In phrase analysis, we speak about the 'head' and the 'dependent'—for example, *delight* and *linguist's*, respectively, in *linguist's delight*. Or consider *two yoke of oxen*, with *yoke* as head and... eh, this seems to be a bit different, though: is *yoke* 'really' the head? Isn't the 'real' head *oxen*? After all, *two yoke* is just a quantifier—a 'periphrastic' one, I like to call it. No, that won't do either, for then one would ask, is that 'real' head, *oxen*, **marked** (by *of*)? Time was when I called this problem (in unpublished diary entries) the 'linguist's despair'—though in class I called it 'head shift', a saner label.

At the time, I didn't know about 'head-marking' languages (Johanna Nichols, who first described them as typologically relevant—*Language* 1986). Those are languages in which they say 'delight-POSS linguist' (not 'linguist-POSS delight'). They never say such things in most of the world's languages, of course, but they would say it like this if they did: or (as they **might** indeed say) 'stars-POSS heavens', or 'funny:accent-POSS field worker'. Now, that would (in a head-marking language) definitely not mean 'the field worker of the funny accent', but 'the funny accent of the field worker'.

So could *of oxen* in *two yoke of oxen* be a 'marked' head? But English is not a head-marking language (take or leave that solitary 3:S:IND:PRES *-s*, and even that only for the subject), and if *of oxen* (with *of*) is the head, then *two yoke* is still not an ordinary attribute: it's a quantifier, and across languages quantifers are sequential nomads: they 'float'.

I ponder, associatively, about 'periphrastic' predicates, those with auxiliaries, like *will come*, or *have done*: *will* and *have* are just that, 'auxiliaries', and *come* or *done* is the 'main' verb. And yet, that 'main' verb is nonfinite, and it is clearly the dependent, for it is the auxiliary which controls nonfinite form (infinitive or participle) in that 'main' verb; and it is the auxiliary which 'agrees with' the subject, changes

tense as needed, and does other things tenses do (e.g., impersonate aspect or mood). Perhaps 'periphrastic' is more apt for predicates, for, after all, periphrastic numerals in English are not many (just with *yoke*, *pair*, and a few others).

Are there other examples of head shift in English? I think of *a prince of a fellow*, *a giant of a man*, *a nightmare of a problem*—where the 'real' heads are *fellow*, *man*, *problem*. But then, those are few also, and have little complementary distribution (**two giants of a man*, **no nightmares of a problem*). Semantically they are expressions of approval or disapproval only, and perhaps for that reason they are nonproductive: no one will say **a generativist of a linguist!* (whether in praise or rejection), or **a Democrat of an Attorney-General!*. So English, it seems, provides little room for head shift—it is stiff-headed.

But is it? Consider *to her*; it is a prepositional phrase—*to* is the head, controlling the *her*-case of *she*. But in *John spoke to her*, *to* belongs to *spoke* more than to *her*—after all, the passive reads *She was spoken to*. So, *to her* as a **phrase** is prepositional, but as a clause constituent it is an argument to the verb: a run-of-the-mill (oblique) NP. Marked, of course, but *to* in that clause is a figure head rather than a real head—the real head is the verb. In some other language, which has already swallowed its former head, a preposition (more likely a postposition, actually), into an oblique case of its own, *to her* would be such an oblique case of that pronoun, and surely no one would say that an oblique noun is 'controlled' just by its obliqueness. According to Nichols, markers, if they migrate, migrate from dependents to heads, across languages—never inversely.

So a stiff-headed language like English may trigger head shift from prepositions to their nouns by attraction of the erstwhile head to its actual next higher head, the verb.

I continue to be tempted to expand the notion of 'auxiliary' from its traditional identity as auxiliary **verb** to other instances of unstable headedness, to include the vicissitudes of more 'classifiers' than *yoke* and *pair*. Consider *information*, which is [–COUNT], so if you want to make it [+COUNT], you need an 'auxiliary' like *item* or *bit*, as in *three items of information*, or *a bit of information*. *Bit* and *item*, which are [+COUNT] as well as pluralizable, are more independent than *yoke* or *pair*, which don't pluralize. So *information* is the 'real' head,

not *bit* or *item*. But then, again, English doesn't have many of those either.

• • •

The notion of 'head shift' suggests a hierarchical arrangement of constituents: one (in a phrase) is the head; the other(s), the dependent(s). So one would be tempted to look for phrases which make hierarchy a problem—phrases in which one constituent is an apposition to the other, as in *my friend John*, or *John, my friend*. But some languages are so hierarchical that when there are two constituents in a phrase they can never be equals. Japanese is such a language, rigidly so. If you want to say 'Mr. Tanaka, my friend' you'll have to say *tomodachi no Tanaka-san* (friend POSTPOSITION Tanaka-HONORIFIC). Are the Japanese, then, saying something like 'Mr. Tanaka of my friend'? Semantically, that makes no sense. But the apposition 'my friend' is an attribute, and attributes in this language can never be equals of the noun they modify.

On the other hand, appositions are true dependents when they are 'restrictive'—the *Charles **the Bald***, not *Charles **the Bold** type*; that is to say, appositions which are uniquely identifying. So *tomodachi no Tanaka-san* could never mean 'Mr. Tanaka—my friend, incidentally' (in English, the addition of a 'sentence adverb' like *incidentally* in an apposition or a relative clause is an error-proof test for nonrestrictiveness). So because Japanese can have appositions only as syntactic dependents, semantically these appositions are necessarily restrictive. The textbook example of attributes which come as restrictive or nonrestrictive is, of course, that of relative clauses. In Japanese, relative clauses are prenominal, and always restrictive. Nonrestrictive relative clauses contain information which is redundant for unique identification of the head noun—a sort of 'afterthought' information. But prenominal nonrestrictive relative clauses would present 'afterthought' information by way of 'forethought': evidently an impossible thing. (A linguist friend tells me postnominal relative clauses are now beginning to emerge in Japanese—this, I suspect, is because of the need to accommodate nonrestrictive relative clauses.)

* * *

So perhaps the openness to head shift in any language depends on how negotiable hierarchical structures are. The hierarchy from head to dependent if the head is a verb or an adposition we traditionally call 'government', and from noun to attribute we call 'agreement'—but for the purposes of my entry today let me consider **this** 'agreement' (i.e. of attributes with nouns) as 'government' also. Then we would have to say that dependent-marking languages have 'government'. But (*pace* Nichols) head-marking languages do not. No case system for arguments (or other clause constituents), no case or other markings for possessor attributes, etc.—all these are marked on heads. Head-marking languages have 'heads' merely semantically, not syntactically; arguments to the verb, and attributes to nouns, are wholly equal coconstituents in a syntax of formal egalitarianism. Ascendancy of heads is merely semantic. Such languages, I hypothesize, have no head shift.

Reference

Nichols, Johanna, 1986. Head-marking and dependent-marking grammar. *Language 62*. ■

HAVE YOU ANY DUST CATCHERS???

Do you have any old linguistic journals lying around collecting dust?
Or maybe you have extra copies of some issues?
Senegal Branch is interested in starting
a collection of general linguistic journals such as
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Dakar, Senegal

Intra-Clausal Movement as a Response to Case Summon

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University of Port Harcourt

There are two types of syntactic movements that NPs can undergo. These are 'Intra-clausal' and 'Inter-clausal' movements. Whereas the reason for the former kind of movement is for Case-checking (i.e. to say Intra-clausal movement is driven by Case Filter requirement), the latter (especially those in which movement is into [SPEC, CP]) is necessitated by some phenomena besides Case-checking (CAK). We argue that if an overt (lexical) NP occurs in a position that is not visible for CAK, it must raise to a more appropriate position, and that all instances of Intra-clausal NP movement are necessitated by this universal requirement. We shall support our claims by examining NP movement in Passive, Ergative and Raising Structures as well as Middles. The model of analysis to be adopted here is Chomsky's (1992) Minimalist Program (MP).[1]

1. THEORETICAL BACKGROUND

Case Theory (CT) is one of the most crucial systems of principles set up by Chomsky (1981) and later modified by him in 1992, to describe the nature of Universal Grammar, and to account for certain specific parameterized variations noticeable in Core Grammars. CT is designed not only to account for the Case features of 'visible' NP positions, but also the distribution of NPs within a construction. Though certain languages (with impoverished morphological inflection) may have nominals which do not show any evidence of overt morphological (Case) declension, there appears to be a consensus in the Government-Binding (GB) or even the MP literature

that every overt NP in any human language must appear in a position that is visible for CAK if the sentence in which such an NP occurs must be well-formed. This attitude gave rise to the formulation of Case Filter, a language universal requirement which states:

1) CASE FILTER
Any sentence which contains an audible nominal element is ill-formed if such nominal occurs in a position that is not accessible for CAK.

Implicit in the above definition of Case Filter is that the 'ability to have or not to have Case depends on the phonological content of a category' (Ndimele 1992:20). Whereas phonetically audible NPs must be found in a position that is visible for CAK, the phonetically inaudible ones may not.

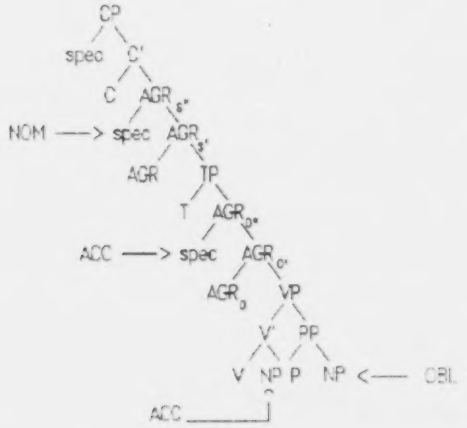
Chomsky (1992) argues that Structural Case is no longer assigned under head-government (= a position which he favored within the GB framework).[2] In MP, however, the core configuration for Structural CAK is the SPEC-Head relation or Head-Complement relation.[3] What this implies is that the notion of government, as far as Structural CAK is concerned, can be dispensed with. Certain construction-specific constraints on CAK for specific nominal positions can now be formulated. For instance, the [SPEC, AGR_S"] (= the specifier position dominated by agreement of subject double bar, i.e. the subject position) of an infinitival clause is not visible for CAK, because its clause is non-tensed. This accounts for no overt NP occurring in that position.

There are essentially three major positions accessible for CAK.[4] These are the [SPEC, AGR_S"] of a tensed clause (= a visible position for Nominative Case-absorption), [NP, VP] (i.e. the complement position of the verb, which is a visible position for Accusative Case-absorption), and [NP, PP] (= the complement position of the preposition, which is an accessible position for Oblique Case-absorption). Another possible position for CAK is the [SPEC, AGR_O"] (= the specifier position dominated by agreement of object double bar). We shall argue elsewhere that a nominal in this position results from the movement of an internal argument of the verb in search of a visible position for CAK, and that the Case feature of this position is also Accusative. In some less inflectional languages, the Case features associated with the complement positions of the verb

and the preposition may not be different. It should also be mentioned that Case association requires strict adjacency of occurrence between the head and its specifier or the head and its complement position in which CAK takes place.

The basic picture for Structural CAK in SPEC-Head or Head-Complement relation is presented thus:

2)



(where CP = Complementizer Projection;
AGR_S = Agreement of subject;
spec = Specifier;
TP = Tense Projection;
AGR_O = Agreement of object;
NOM = Nominative Case;
ACC = Accusative Case;
OBL = Oblique Case).

The position for CAK for a particular nominal is dependent upon Case directionality. If the Case feature of a verb or a preposition, for instance, is to be checked to its right, there must be an argument to its right to assume this Case property, and again if the Case feature of a head of some kind of projection is to be determined to its left

(e.g. AGR_S or AGR_O), there should be an argument to its left to acquire the associated Case feature.

2. INTRA-CLAUSAL VS. INTER-CLAUSAL MOVEMENT

There are essentially two types of movement in which constituents may be involved. These are Intra-clausal and Inter-clausal movements. When a constituent is moved from one syntactic position into another position within the same clause, this is described as Intra-clausal movement, while the movement of a constituent out of its original clause is referred to as Inter-clausal. In other words the difference between the two types of movement is dependent upon the domain of movement (i.e. whether a clausal wall is crossed or not). We argue that Intra-clausal movement is triggered by the desire for a nominal to appear in an environment where CAK is feasible, while Inter-clausal movement involving an NP or any other category (especially if movement is into a non-argument position) is triggered by certain other phenomena outside CAK. The reason why we argue that Inter-clausal movement is not a response to Case Filter requirement is that the [SPEC, CP] (= the specifier position dominated by the complementizer projection), which is almost always the ultimate landing site for any preposed constituent involved in an Inter-clausal movement, is not a transparent position for CAK. In other words, CP is an opaque domain.[5]

In what follows, we shall examine some cases of Intra-clausal movement resulting from Case Filter requirement.

2.1 NP MOVEMENT IN A PASSIVE CONSTRUCTION (PC)

A PC is a type of clause where the grammatical (superficial) subject of the sentence is the Theme or the recipient of the action expressed by the verb. Passivization involves a movement of the underlying internal argument of the verb into an empty [SPEC, AGR_S"].

Let us examine the following examples:

3) a. [$_{NP}$e] is industrialized the country.
   b. The country$_j$ is industrialized t_j.
   b'. CH = [the country, t]
4) a. [$_{NP}$e] must have been stolen the ball.
   b. The ball$_j$ must have been stolen t_j.
   b'. CH = [the ball, t]

In (3a), for instance, we observe that 'the country', which is the underlying internal argument of the verb, is moved into the subject position of the same clause from where it enters into a Theta chain with its trace at the original extraction site. The ghost copy of the preposed constituent and the preposed constituent itself are assigned a unique subscript index by virtue of shared grammatical features.

The position for CAK or Theta role assignment in a PC differs from that of a non-Passive one. Whereas the core configuration for determining both Theta role and Case for an internal argument of an active verb is [NP, VP], only a Theta role can be determined in such a configuration for an internal argument of a passive verb. In other words, the positions for the determination of Theta role and Case do not coincide in a PC.

There are two obvious implications in the claim that the internal argument position of a passive verb is a domain for Theta role assignment, but not for CAK. The first is that the Case feature of the internal argument of a passive verb must be determined elsewhere in fulfillment of the Case Filter requirement. Secondly, the subject position into which the original internal argument of the passive verb moves must be dethematized, so that the preposed NP does not acquire another Theta role in its new home. An explanation which we can proffer for the inability of the preposed NP to take on another Theta role in its new home may be that Theta role assignment is a D-structure requirement which must take place before movement. In order for the preposed constituent to have a Theta role, its associated role in the original extraction site is transmitted to it via a movement chain in accordance with 'Chain Transmission Principle', which states that grammatical properties are freely transmitted between an antecedent and its trace. What this means is that Move-Alpha does not change the semantic values or properties of its affected constituents. This explains why each movement chain (cf. (3b') or (4b')) carries a unique set of grammatical features (e.g. one Theta role, one Case, etc.).

The chain relationship demonstrated in (3) or (4) above is an A-chain (= Argument chain). An A-chain differs from a non-argument chain. An A-chain is an antecedent-anaphora relation in which the antecedent occurs in an argument position. A constituent which serves as the antecedent of trace is the 'Head' of the chain, while the

trace that occupies the extraction site is the 'Tail'. Any two adjacent constituents in a chain constitute a 'Link'.

Although we had earlier said that Passivization involves the movement of an underlying internal argument of the verb into the [SPEC, AGR_S] (= the subject position), movement of an argument out of a double object construction presents some difficulty, as can be seen below:

5) a. [$_{NP}$e] was given John an egg.
   b. John$_j$ was given t_j an egg.
   b'. CH = [John, t]
   c. *An egg $_j$ was given John t_j

Sentence (5b) rather than (5c) is grammatical. The ungrammaticality of (5c), according to van Riemsdijk and Williams (1986), results from the fact that the extracted constituent, 'an egg', does not originate immediately to the right of a 'Natural Predicate'. Ndimele (1992:60), however, contests the whole idea of the natural predicate requirement as far as NP movement in a PC containing more than one argument is concerned. In fact, the whole notion of natural predicate is a misnomer.

Let us examine the following:

6) a. [$_{NP}$ e] was given an egg to John.
   b. An egg$_j$ was given t_j to John.
   b'. CH = [an egg, t]
   c. *John$_j$ was given an egg to t_j
   d. *To John$_j$ was given an egg t_j

From the above examples, it is not clear what the natural predicate is. What happens rather is that an NP that moves in a PC must originate immediately to the right of the passive participle. Ndimele (1992) analyzes this situation in terms of 'Proximity Condition' rather than van Riemsdijk and Williams' (1986) Natural Predicate Hypothesis. According to Ndimele, the NP that moves in a PC must originate immediately to the right of the passive participle in accordance with the Proximity Condition.

7) PROXIMITY CONDITION
No NP in a Passive construction can be moved across another NP that is structurally the immediate right sister constituent of the passive morphology. In other words, an argument position that is not structurally next to the right of the passive verb is invisible to Move-NP.

It is this Proximity Condition that accounts for why 6(c)-(d) rather than (6b) are ungrammatical.

2.2 NP MOVEMENT IN AN ERGATIVE CONSTRUCTION (ERC)

Johns (1992:57) observes that a language displays Ergativity (Erg):

> ...when rules of case assignment and/or agreement treat the subject of a transitive clause differently from the subject of an intransitive clause and the object of a transitive clause...

It has also been observed that in some languages that display Erg, the Actor in a transitive clause is the D-structure internal argument. In fact, the identification of the grammatical roles of constituents (or the notion of dominance relation; e.g. [NP, VP] or [NP, AGR_s-]) is crucial to the investigation of constructions exhibiting Erg. Johns (1992:60) locates the source of Erg in Inuktitut (Canadian Eskimo) 'in three independent, but interacting, language-specific properties' including 'the inability of the verb to project to a VP'. What is implied here is that any argument of a transitive verb must be projected externally to the VP at S-structure as well as D-structure. But our position here is that every V node in any language should have the ability of projecting to a VP (passing through some kind of recursive intermediate phrasal node(s) where applicable). A crucial reason why certain verbs may seem not to permit the presence of an overt/surface internal argument or why some underlying internal arguments of the verb are obligatorily found in the external argument position may be because the complement position of such verbs are not accessible for CAK. This is exactly what happens in the case of ERCs.

An ERC is an instance of monadic predicate. It is a semantic characterization of an ERC that its verb is not only a one-place predicate, but also unable to assign a subject Theta role.

Let us examine the following sentences:

8) a. [$_{NP}$ e] melted the wax.
   b. The wax$_j$ melted t_j
   b'. CH = [the wax, t]

9) a. [$_{NP}$ e] sank the boat.
   b. The boat$_j$ sank t_j.
   b'. CH = [the boat, t]

10. a. [$_{NP}$ e] baked the bread.
    b. The bread$_j$ baked t_j
    b'. CH = [the bread, t]

The verbs in pairs like (8a,b), (9a,b) and (10a,b) are referred to as Ergative verbs, and the sentence pairs as Ergative pairs. From the above examples the following observations can be made:

i) Erg is a productive rule whose transitive member (cf. (8a), (9a) and (10a)) serves as the input to generate the surface intransitive member (cf. (8b), (9b) and (10b)).

ii) A movement transformation applies to extract the internal argument of the transitive member (cf. (8a), (9a) and (10a)) of an ergative pair to create a trace at the extraction site. The trace left behind by the preposed internal argument is subject to ECP (= Empty Category Principle), a grammatical requirement which states that traces must be 'properly governed' (i.e. antecedent-governed).

iii) The Theta role of the proposed constituent is the same (i.e. Theme) in both the source and target positions. This is a pointer to the fact that the [NP, AGR$_s$"] in an ERC does not receive a Theta role unless through inheritance from its source position.

iv) The movement of the internal argument into the subject position in each of the (b) sentences above is in fulfillment of the Case Filter requirement.

Another interesting thing about the behavior of Ergative structures is that the trace elements left behind by the preposed internal arguments have the effect of barring the presence of a nominal element in the complement position of the verb of the sentence. This accounts for the ill-formedness of (12b).

11) a. The boy ran.
    b. The boy ran a race.

12) a. The ball$_j$ bounced t_j
    a'. CH = [the ball, t]
    b. *The ball$_j$ bounced t_j a bouncing.

The ill-formedness of (12b) can, as well, be explained from the point of view of CAK. In fact, the complement position of an ergative verb is not a position for CAK, hence any overt NP in that vicinity will violate the Case Filter.

An interesting point to mention is that an ergative verb differs from a pure intransitive verb in the sense that the former may have an underlying internal argument while the latter does not. The underlying internal argument manifests as the superficial subject of the ERC. Ergative and intransitive verbs, however, differ from the transitive ones in the sense that verbs in the first category (cf. (13) and (14)) can prepose, while those in the second (cf. (15)) cannot.

13) a. The weeping child [Intransitive]
b. The sleeping dog "

14) a. The melting wax [Ergative]
b. The sinking boat "

15) a. *The killing goat [Transitive]
b. *The sweeping house "

2.3 NP MOVEMENT IN A MIDDLE SENTENCE (MS)

Another construction which involves an obligatory externalization (via movement) of the internal argument of the verb into [SPEC, AGR_S"] for purposes of CAK is the so-called MS. Fagan (1988), however, argues that Middles are derived lexically and not syntactically. She stresses that since p(repositional) stranding is prohibited in middles, the externalization of the internal arguments of the verb cannot be explained by any kind of movement operation. Our contention is that the absence of P-stranding is not enough reason to reject the adjunction-oriented hypothesis for the externalization of the internal argument of the verb. Our position is that the [NP, PP] position is a crucial relation for CAK, and that the verb in an MS cannot subcategorize for such a position. That MS can permit neither P-stranding nor a case where the preposition and its complement are not separated within the VP is a case against Fagan's position and, in fact, a pointer to the fact that the predicate slot of an MS does not harbor any visible A-position (= argument position) for CAK.

Although it is argued in the literature that Middles have a lot in common with ERC to the extent that it is sometimes difficult to draw

a neat line between the two, there are certain subtle differences between them, as can be seen below:

16) a. The stone$_j$ broke t_j [Ergative]
b. The stone $_j$ breaks t_j easily. [Middle]

17) a. The bread$_j$ baked t_j [Ergative]
b. The bread$_j$ bakes t_j easily. [Middle]

18) a. The boat is sinking. [Ergative]
b. Sink, this boat! [Ergative]

19) a. *The bureaucrats are bribing easily. [Middle]
b. *Read poorly, this book! [Middle]

20) a. The boat sank all by itself. [Ergative]
b. *The door opens easily all by itself. [Middle]

From the above examples, the following conclusions can be made:

i) Both the ERC and MS involve some kind of Argument Promotion from the internal to external argument position leaving behind a coindexed trace at the source position.

ii) The positions for CAK and Theta role specification are the same in Ergative and Middle constructions.

iii) Whereas the Middle verb obligatorily requires the presence of a postverbal adjunct phrase, the Ergative one does not.

iv) Unlike Ergatives, Middles cannot occur in progressive or imperative constructions. This accounts for why 19(a)–(b) are ill-formed.

v) Middles can only be expressed in the simple present.

vi) The trace left behind when the underlying internal argument is preposed in an MS is also subject to ECP.

vii) Middles, unlike Ergatives, are 'generic sentences' (cf. Keyser and Roeper 1984) which do not describe events, 'but attribute properties to objects that hold regardless of time' (Fagan 1988:201-202).

viii) Whereas Middles, according to (Keyser and Roeper 1984), 'seem to retain an implicit agent, ergatives do not' (cf. 20(a)-(b)).

ix) The trace element confers the thematic role (Theme) on the subject by virtue of coreference.

2.4 NP RAISING INTO [SPEC, AGR$_S$"]

It is particularly interesting to note that in Echie (an Igboid lect spoken in the Southern part of Nigeria), any internal argument that

follows an Obligative Participle (OBP) form of the verb must obligatorily move to the immediate left of this verbal derivative, as can be seen in the following examples:

21)a. Ézè gà [NPe] ń-rí !jí.
(Eze FUT -- OBP-eat yam)

b. Ézè gà jí$_j$ ńrí t$_j$ 'Eze will eat some yam'.

b'. CH = [ji, t]

c. *Ézè gà ńrí !jí.

d. Ézè gà í-!rí jí. 'Eze will eat some yam'.
(Eze FUT to-eat yam)

e. *Ézè gà jí í!rí.

Sentence (21a) is the underlying form with an empty NP position immediately to the left of the OBP and the internal argument to its right. We argue here that for purposes of CAK, the internal argument raises into a possible empty NP position that is more accessible for CAK. In the case of (21b), the object raises into [SPEC, AGR$_O$"] leaving behind a coindexed trace at the source position. For this movement operation to be possible, the verb, *ga*, raises into AGR$_s$ thereby making it possible for the raising of the object into [SPEC AGR$_O$"] for CAK.[6] The canonical order of words in Echie is SVO, hence one would have expected (21c), in which the object occurs to the right of the OBP, to be well-formed. The reason why (21c) is ill-formed may be that any verb in Echie that takes the OBP Marker, *N*=, automatically gets deverbalized, and therefore can never stand in Head-Complement relation with an internal argument, which is a core configuration for CAK. In fact, the affixation of the OBP in Echie is a category-changing operation capable of rendering the original internal argument position totally non-transparent for CAK. (21d) is well-formed, because the complement position is transparent for CAK, hence the object subcategorization does not move into [SPEC, AGR$_O$"]. This accounts for the oddity of (21e).

3. CONCLUSION

We have argued that NP-raising into a position within the same clause is driven by Case Filter, and that if the Case feature of an NP has already been checked, then movement is not necessary. In all the cases we investigated, we observed that the [NP, VP] for these constructions is not visible for CAK. In Ergative, Passive and Middle

constructions, there is a general failure of the VP projecting its associated external Theta role into the [SPEC, AGR_S"] position, hence this position is dethematized. Our discussion here has some implications principally for CT, and minimally for subcategorization, indexation, ECP, transformations, Theta Theory, etc. Finally, the similarities we noted in the constructions investigated are traceable to Case-absorption.

NOTES

1 Minimalist Program is a framework which was considered by Chomsky (1989), but formalized in 1992 in a monograph titled *A Minimalist Program for Linguistic Theory*. MP, as proposed by Chomsky, is supposed to be an improvement on GB. In fact, the whole essence of MP is to achieve some economy in derivation. The following quotation from Chomsky himself lends credence to the above assertion: "UG provides a fixed system of principles and a finite array of finitely-valued parameters. The language-particular rules reduce to choice of values for these parameters" (Chomsky 1992.5).

2 It is argued in GB Theory, however, that Structural Case is assigned under government. In other words, ungoverned positions are not visible for Case-marking. This explains why the [SPEC-I'] (= the specifier of Inflection bar, i.e. subject position) of an infinitival clause which contains an inaudible category, PRO, escapes Case Filter.

3 Chomsky (1992:11) believes, however, that "the SPEC-head and head-head relations are the core configurations for inflectional morphology".

4 In GB Theory also, there are essentially three major governors that are responsible for Structural Case-assignment. They are the [+ Tense] INFL (= tensed Inflection, which assigns a Nominative Case to an NP in the subject position of the sentence), the verb (which assigns an Accusative Case to its internal argument), and the preposition (which assigns an Oblique Case to its complement).

5 The popular view within the GB literature is that CP is not in the domain of any governor. In fact, neither C (= complementizer) nor any other category housed within CP can be governor. CP forbids any category which it harbors to be governed.

6 We wish to argue that the preposed internal argument occurs in Head-Complement relation with the verb, *ga*, which may be admitted as a core configuration for Structural CAK.

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Adverbial Clauses and Topicalization in Me'en[1]

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1. Introduction

In Me'en, a Southern Surma language spoken in the southwest of Ethiopia, there is only one subordinating suffix, and it is unmarked regarding time or logical relationships. In addition, it is remarkable that subordination is hardly ever found outside of texts.

2. Adverbial clauses

The subordinating suffix is *-ɔ́*, and it is suffixed to adverbial clauses which always precede the main clause.

(1) šubúnɛ́-díáɲ bí tal -ɔ́, joy k'úrša m̂ɛ́ri.
father-my cow sell-SUB find money much
When/because/if my father sells a cow, he gets a lot of money.

As the translation of (1) already shows, the subordinate clause may be interpreted in several ways: as a when clause, as a reason clause, or as a conditional clause.

Thompson and Longacre (1985:181f,193) report on a number of neutral subordinators in other languages, e.g. a time subordinator in Wappo that also serves to mark cause. In Mursi, a closely related Surma language, Turton and Bender (1976:551) gloss the subordinator *huli* with 'when/if', but they don't give any further comments.

2.1 Clues for decoding implied relationships

If adverbial clauses are unmarked as far as subordinators are concerned, how can the hearer select the correct semantic interpretations where he/she always hears the same surface forms? Certain clues help decode the implied relationships.

2.1.1 Context clue

There is always the immediate context which helps to define the implied relationships. The normal interpretation of (1) above will be the 'when' clause. In case there is some reason in the preceding text, e.g. that money is needed, the sentence will be understood as 'If he sells a cow, he will get a lot of money'; on the other hand if the action is in the past and the argumentation centers on the source of the money, the 'because' clause will be understood.

2.1.2 Aspectual clues to differentiate time clauses

Aspect markers also serve to determine the relationship between clauses in a sentence. They help to differentiate 'after' and 'while' clauses from other temporal interpretations.

In all the Me'en text-material collected so far, in subordinate constructions with the first clause in perfective aspect and the second unmarked, 90 percent were clearly understood as 'after' clauses (the remaining as 'if' or 'because', determined by context). In these subordinate clauses the perfect suffix *-áá-bóy* marks the action as prior to that of the second clause. The following examples are illustrative:

(2) gaynátá ar -t -ááb**óy** -ɔ́, háy -oŋ.
relatives see -PL -PERF -SUB go -NEG
After the relatives have seen (him), they don't leave.

(3) ḿɛ́ɛ bɔk -áá k'ěr -b**óy** -ɔ́, mat šólu.
man plow -PERF field -PERF -SUB drink beer
After the man has plowed the field, he drinks beer.

In (2) the relatives don't leave after they have seen the man, and in (3) the drinking follows the plowing. The perfect marker in both subordinate clauses merely functions with reference to the respective

main clause, while both sentences as such may be determined as past, present, or future by their context.

For 'while' clauses, Me'en uses the progressive marker *-íné* which indicates simultaneity of the actions of the two clauses—a strategy also reported from other languages (Thompson and Longacre, 1985:189).

(4) nén am -íné gi?ec -ɔ́, dɔ?ɔŋ tɛp tuí -ɔ́ŋɔ≤
he eat -PROG bone -SUB that-one watch house -that
While he was eating the bone, the other one watched the house.

2.1.3 Negative adverb to mark 'before' clauses

Since the action of the 'before' clause does not take place before that of the second, *ane* 'not yet' is used—a strategy also reported for Quechua (Thompson and Longacre, 1985:183).

(5) mɔkác áné rɔ?ɔ́n -áábóy -ɔ́, dɛší c?ɔ́llɔ.
woman not yet give birth-PERF -SUB work always
Before the woman gives birth, she works all the time.

2.2 Explicit subordinating conjunctions

Explicit subordinating conjunctions are very rare. They are clause-initial and can occur either with or without *-ɔ́* at the end. All are composites: *séy ke* 'when' (lit.: say like). *bállé* 'time of', *báállɔ́nɔ* 'when' (lit.: time-that) and *boydíá* 'because' (lit.: place-this).

(6) boydíá gúrit ɗaa tommogít -téné, nén šuré máɗák.
because thief take money -his he anger very-much
Because the thief took his money, he is very angry.

(7) séy ké gɔrɔ́ -gé-ɔ́, cuc bí.
say like road -on-SUB butcher cow
When he (was) on the road, he butchered a cow.

3. Topicalization and subordination

Besides its use as subordinator, the suffix *-ɔ́* also marks a topic ('topic' in the sense of 'given or old information'). Narratives often

begin with *iláŋá kíŋuɲɔ́, iláŋáníɔ́*, or *kíŋuɲɔ́* a long time ago, in former times, etc., where the final *-ɔ́* topicalizes this temporal setting.

Other parts of speech can also be topicalized, as in the following examples:

(8) de iké koy -ná maɲ kómáj -a -gé -ɔ́, c'uwá
REL like go -and rob strength-PL -INSTR -TOP then
Concerning the one who goes and robs forcefully,

dáyɲa kóy -ná, maɲ bí -dé mɛ́nd-uɲ -ná, ac mɛ́n-dɔŋɔ.
judge go -and rob cow -REL man -GEN-and give man-that
the judge will go and take his cow and give it to the other man.

(9) woškít -ɔ, c'uwá g -ulúp buté -gé apaláy.
bark -TOP then 1PL -wear instead -REL clothes
Concerning bark, that's what we were wearing instead of clothes.

In both cases above, the topicalized item is something that has already been mentioned before and then is expounded further in the clause following the topicalizer. The question is now whether this suffix marking topicalization is identical with the subordinator *-ɔ*.

Phonologically there is no difference. In both cases the *-ɔ* is marked with the same high tone and followed by a clearly audible pause. The presence of a phonological pause has been observed to be a condition for topics (Foley and Van Valin 1984:125). Topicalization and subordination also share common functional features.

In Me'en discourse two successive paragraphs are often linked by 'tail-head linkage' to provide cohesion, a kind of back-reference where the last part of the preceding paragraph is repeated at the beginning of the following paragraph.

(10a) kúŋkula -ná ó káŋac ɓɔk-á gacít.
mouse -PL and monkey sow-PAST millet
The mice and the monkey were sowing millet.

(10b) ɓɔk gacít -ɔ́, kúŋkula ó káŋac udkúm -á...
sow millet -SUB mouse and monkey pick -PERF
When they sowed millet, the mouse and the monkey picked...

(10a) is the first paragraph of the story, 'The Mice and the Monkey'. (10b) introduces the second paragraph of that text by repeating the last clause of the first paragraph. The adverbial clause there is marked by the subordinator *-ś*, but if we compare the adverbial clause of (10b) to the topics above, and if we consider that it fulfills requirements for topics, e.g. sentence-initial position, discourse dependence, definiteness, etc. (Chafe 1976:50), we can assume that this same *-ś* functions as topic here. Note that Thompson and Longacre (1985:229) point out the relationship between topics and subordinators when adverbial clauses are used in discourse to maintain cohesion.

As noted above, subordination is restricted to texts. In elicitation hardly any subordinate clause with the *-ś* suffix is found, and explicit subordinating conjunctions as in 2.2 are only rarely given. Coordinated constructions are usually supplied instead. Here again the first clause is unmarked.

(11) nên koy gába -y -ná, tal bí kóón.
he go market -DIR -and buy cow one

(11) could be interpreted as:

a) He goes to the market and buys a cow.
b) When he goes to the market, he buys a cow.
c) Because he goes to the market, he buys a cow.

4. Conclusion

It was very puzzling that coordinating constructions were supplied in elicitation while subordinating sentences are standard in texts. The solution is that a relationship beyond homophony exists between the subordinator and the topicalizer; they are the same. Now the restriction of subordination to texts can be explained. In texts, the conditions of discourse dependence and definiteness are met, but not in elicited material, which is restricted to isolated syntactic constructions.

Haiman (1978) has given a number of valid arguments to prove that a certain kind of subordinated clause can be viewed as topics in an article called 'Conditionals are topics'[1]. He not only reports a number of languages where conditionals and topics are marked by the

same surface form (as we have seen for Me'en), but he also points out clear semantic and pragmatic parallels between the two.

Haiman elaborates a definition of topics that is very close to his definition of conditionals. Here only the former is quoted:

> The topic represents an entity whose existence is agreed upon by the speaker and his audience. As such it constitutes the framework which has been selected for the following discourse (ibid. 585).

'Framework... for the following discourse' closely captures what is common to the subordinated clauses and the topics in the Me'en examples above. More research on topicalization, subordination, and temporal and logical implicitness[3] as shown in section 2 is needed. The study of other Nilo-Saharan languages could provide more insights.

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Notes

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[2] Fritz Serzisko made me aware of this article, for which I here express my thanks to him.

[3] One further observation should be noted here. It has been mentioned in the literature, e.g. Haiman (1978:570), that there is a relationship between interrogatives and subordinators/topics. In Me'en there is no direct correspondence between any of the interrogative markers and the subordinator/topic marker, but often a clause is topicalized before a final question-word:

(12) án -de nèb gub -oŋ -ś, á táŋ?
thing-REL he want-NEG-TOP, is what
What is it that he does not want?

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Some Reflections on Formal Syntax

David Weber

(submitted June 1991)

We who may be primarily interested in functional and typological linguistics should keep an eye on what is going on in formal linguistics. I am particularly concerned that the Summer Institute of Linguistics is increasingly turning its back on formal linguistics—even to the point that some who study formal linguistics feel out of place.

Some of my coworkers think I am crazy to study formal grammar—that, as King Agrippa said to the apostle Paul, "You are out of your mind! Your great learning is driving you insane!" (Acts 26:24). Others would remind me of King Nebuchadnezzar, who the prophet Daniel reports as saying: "I, Nebuchadnezzar, raised my eyes toward heaven and my sanity was restored" (Dan. 4:34). They feel that if I would only raise my eyes toward heaven—and study linguistics as it has been revealed through certain prophets—then my sanity would be restored.

Now I do not intend to suggest that we abandon the study of typological and functional linguistics. Rather, I wish to suggest that formal linguistics is becoming more interesting and worthwhile, and that it is in our interests to study it. I feel that the best linguistics of the future will pull together functional/typological linguistics and formal linguistics. Bernard Comrie (1988, p. 460) is of this mind:

> Nonetheless, I believe that the convergence of interests between practitioners of linguistic typology and generative syntax has the potential to create important new advances in both fields. By this I do not want to advocate mindless eclecticism—a good approach does not arise through mixing several poor approaches. In this contribution I have tried to show the extent to which good work in linguistic typology is dependent on developments in general linguistic theory, and I believe firmly that genuine interaction and mutual respect between linguistic typology and grammatical theory will lead to significant advances in our overall understanding of language, which is surely the goal of all linguists.

Encouraging developments

One reason to feel optimistic about a greater integration of formal and functional/typological linguistics is that Chomskian linguistics is shifting from being an almost exclusively structural enterprise to one in which functional and typological factors are incorporated. Let me mention some signs of this shift:

1. Cross-linguistic diversity is to some extent incorporated into the theory by means of *parameters*. Some of these are clearly typological in nature. For example, one parameter indicates whether a language is head initial—roughly correlating with the typologists VO type—or head final—corresponding to the OV type.

2. The phenomena of 'pro-drop', that is, whether the subject can be dropped in simple clauses, has a functional explanation in terms of whether subject agreement is rich enough to support reference without an overt subject NP.

3. For many years, when research was heavily influenced by English, only highly configurational clause structures were admitted. An S was treated as an NP and a VP. The VP was a V, with perhaps an NP (if transitive), and perhaps a complement or other element (PP, AdvP, etc.), somewhat along the lines of a in Figure 1 below.

   Hale (1983) introduced the idea that some languages—which he called 'non-configurational' languages—had very flat phrase structures, along the lines of b in Figure 1 below. Chomsky (1986, p. 3) admits the three possibilities diagrammed in Figure 1, among others.

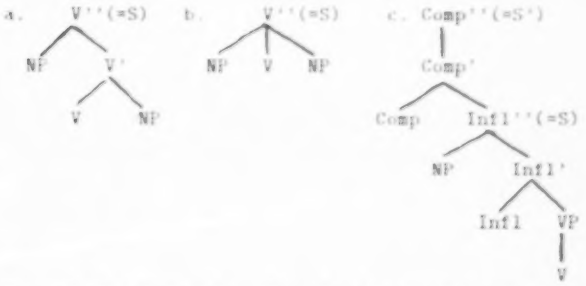

Figure 1. Some possible configurations for sentence structure

Chomsky (1989) suggests yet another alternative (following work by Emonds and Pollock (1989)) the explicit intention of which is to better accommodate different languages, such as those in which verbs agree with their objects.

Those who think that formal linguistics have tried to press languages into a mold created for English should reflect: These possibilities demonstrate a recognition that languages may differ even in the fundamental aspects of clause structure.

4. Government and Binding (GB), Lexical Functional Grammar (LFG) and Generalized Phrase Structure Grammar (GPSG) (as well as its daughter, Head-driven Phrase Structure Grammar (HPSG)) share a common conception of structure—called X-bar theory—the central idea of which is that phrasal categories are projected from heads.

   X-bar theory provides little more than a structural skeleton to which other theories or components add meat. For the most part, X-bar theory is a weaker theory of structure than its predecessors, although it has a richer and more interesting theory of categories.

   Kornai and Pullum (1990) argue that X-bar theory is for the most part a vacuous theory. That should encourage us: The structural part of the overall theory is doing virtually nothing! It is right and fitting that answers not be sought in terms of structure per se, but in terms of the principles and parameters that constrain structures. The parameters have a definite typological flavor about them, and many of the constraining principles have a genuine functional flavor.

5. Increased importance is given to the lexicon and particularly to the 'argument structure' of verbs. Stowell's (1981) proposal to altogether abandon phrase structure rules in favor of an enriched notion of the lexicon has been widely accepted.

6. Transformations defined in terms of specific structural conditions have been abandoned in favor of move-α. This has shifted the focus further from structure toward the principles and parameters governing structure. At first, move-α dealt almost exclusively with nominal elements; it amounted to saying "you can move any nominal element anywhere provided you don't violate a principle". Now move-α is applied to virtually any category; in particular, verbs and pre/postpositions can be moved. The result is that rather than structure being seen as something rigid, it is regarded as quite fluid. Individual structures may seem rigid, but that is because principles and parameters keep them that way, not because the structure is inherently rigid.

7. Not only is the current approach less structural, it is also less abstract. Starting with his 1970 article, "Remarks on Nominalization" (which put an end to abstract, transformational analyses of lexical nominalizations), Chomsky has repeatedly made proposals that reduce the degree of abstraction. For example, in a recent proposal Chomsky (1989) explicitly seeks to reduce the distance between underlying and surface forms.

Structure and inaudibilia

My linguistic education has been largely from a surfacist perspective. I learned that syntactic units (sentences, clauses, phrases) are strings of words (morphemes) under an intonation contour. Syntax deals with morphemes, order, proximity and intonation. Positing 'things' for which there is no physical evidence was regarded as unscientific. The rule was, "What you see is what you get" or (to put it more accurately) "What you hear is all you got".

Weber (1983) was written from this perspective. Perhaps its least satisfactory aspect was its treatment of reference. For a range of cases discussed below, I attempted to explain the reference of certain gaps exclusively in terms of precedence and proximity.

Now Givón (1983) has demonstrated that, as a general tendency, the closer a reference point is to its antecedent, the smaller it will be. If it is very close, it is more likely to be a gap, while if it is far, it is more likely to be a full noun phrase. Pronouns generally lie somewhere in between.

Givón's theory captures something very real about reference, particularly with respect to reference between sentences. However, within sentences other factors must be taken into account. Consider the following Huallaga Quechua example:

1) *Wamraqa... pushaku-n* mama-n-*ta* mama-n *tiya-sha-n marka-pa*
child leads-3 mother-3p-obj mother-3p live-sub-3p town-gen
The child... leads his mother to the town where his mother (had) lived.

Why isn't the second case of *mama-n* a gap? After all, it co-refers to the immediately preceding word. The answer is that, although it is **physically** close, it is **structurally** distant. The verb phrase has the following structure:

2) $[_{tP}\ [_{tP}$ *pushakun mama-n-ta*$]\ [_{PP}\ [_{NP}\ [_{S}$ *mama-n tiyashan*$]$ *marka*$]$ *-pa*$]]$

Example 3 is similar, provoking us to ask, "Why is *pay* 'he' used rather than a gap? After all," we might say, "*pay* usually refers to someone **other than** the most immediate referent."

3) *Mana perdunashqatsu Josë pay -ta rantikushqan -ta*
not did.not.forgive Joseph him obj that.they.sold obj
Joseph did not forgive them for having sold him.

Again, the reason is that—although *pay* immediately follows *Josë* in the physical stream of speech—structurally it is quite far from *Josë*:

4) [$_S$ *Mana* e_i *perdunashqatsu Josë*$_j$] [$_{PP}$ [$_S$ e_i [$_{VP}$ *pay*$_j$ *-ta rantikushqan*]] *-ta*]
not did.not.forgive Joseph him obj that.they.sold obj
Joseph$_j$ did not forgive them$_i$ for (their$_i$) having sold him$_j$.

Examples like these convinced me that constituent structure must be taken into account to understand the occurrence of gaps, pronouns, and full noun phrases.

Not only is it necessary to understand constituent structure, it is also necessary to grapple with 'inaudibilia', like the empty categories represented by *e* in Example 4. I used to be offended by inaudibilia and all the fine-tuned argumentation about them; it was hard to be interested in their classification into 'anaphors', 'pronouns', 'PRO' and 'pro'. I now believe that grammatical theory makes interesting, accurate predictions about gaps[2] and that we cannot really understand reference unless we face up to their existence. They are not a worthless nuisance—as I once thought—but a vital resource for understanding how language works.

We can make mistakes if we do not acknowledge that some things are present psychologically although they have no physical realization in the speech stream. For example, Stewart (1987, p. 283) claims that 5 is a switch reference violation:

5) *Waqa-pti-ki-lla-qa-m maldiciona-yka-ma-nki-man-pis*
cry-ds-2-just-top-dir curse-up-1-2-cond-even
Just by crying, you might even place a curse on me.

She writes:

> Although the subjects in the two clauses of 5 are co-referential, *-pti* ['different subject'—DJW] marks the first clause, another clear violation of syntactic principles of switch reference marking.

But I will show that there is a completely reasonable perspective from which 5 is not a switch reference violation.

In Quechua generally, when *ka-* 'to be' is used to support a predicate complement, it does not surface if it is present tense and has a third person subject. For example, we would say

Qam hatunmi	ka-nki	'you are big' (with *-nki* 'second person')
Qam hatunmi	ka-rqa-n	'he was big' (with *-rqa* 'past tense')
Pay hatunmi	_____	'he is big'.

The only cases in which *ka-n* (be-3) surfaces is in existentials such as *yaku ka-n* 'there is water'.

Conditional verbs are compound constructions formed with *ka-*, which surfaces in tenses other than the present. For example, consider the following:

6) *Waqa-pti-ki$_i$-lla-qa-m* *maldiciona-yka-ma-nki-man-pis* *ka-rqa-n$_j$*
cry-ds-2-just-top-dir curse-up-1-2-cond-even be-past-3
Just by (your$_i$) crying, you might have even placed a curse on me.

The syntactically main verb is *karqan*, even though it is not the semantically more prominent. (This is what we expect since Quechua is a head-final language.) The subject agreement marker on this verb is third person, whereas that of the switch reference clause is second person. Therefore, the subject of the main verb is not co-referential with the subject of the adverbial clause and the different-subject marker on the subordinate clause is correct.

Now let us return to Example 5, repeated as 7:

7) *Waqa-pti-ki$_i$-lla-qa-m* *maldiciona-yka-ma-nki-man-pis* *KA-N$_j$*
cry-ds-2-just-top-dir curse-up-1-2-cond-even be-3
Just by crying, you might even place a curse on me.

In Example 7 I have included the understood but inaudible *KA-N$_j$* to make it clear that the subject agreement marker is not co-referential with the subject of the adverbial clause. The different-subject marker is correct. This is not a switch reference violation.

The moral: Recognizing inaudibilia allows us to explain cases that would otherwise be exceptions.

Sensory verb complements

Let me show you some data that convinced me that we must study constituent structure and inaudibilia. Huallaga Quechua has rather free constituent order. Further, in the complements to sensory verbs (for example, 'see', 'hear', 'sense'...), the subject or the object of the complement can move into the higher clause. I conducted a brief study by making a form with a couple dozen ways to say "John saw Tom hitting his child". I gave these to Quechua speakers, asking them to judge whether **his son** referred to **John's** son or **Tom's** son. For some cases there was definite consensus; for others, there were divergent opinions; some speakers expressed that it could go either way; others found it very hard to make any judgment. But their judgments were collectively very instructive.

In Example 8 *wamran* clearly refers to Tom's son; the *-n* of *wamra-n* refers to *Tumas*, as indicated by the subscripted index:

8) *Hwan Tumas$_i$ wamra-n$_i$-ta maqa-yka-q -ta rika-ra-n*
John Tom child-3p-obj hit-impf-sub obj see-pst-3
John saw Tom hitting his child.

The structure is as follows: [3]

[$_S$*Hwan* [$_{PP}$[$_{S,NP}$ *Tumas$_i$* [$_{VP}$*wamra-n$_i$-n-ta maqa-yka-q*]] [$_P$*-ta*]] *rika-ra-n*]

Likewise, for Examples 9 and 10 *-n* clearly refers to *Tumas*:

9) *Hwan rika-ra-n* [*Tumas$_i$* [*wamra-n$_i$-ta maqa-yka-q-ta*]]

10) [*Tumas$_i$* [*wamra-n$_i$-ta maqa-yka-q-ta*]] *rika-ra-n Hwan.*

But in Example 11, where *wamra-n* occurs in the main clause,[4] it refers to John's son:

11) *Hwan$_j$ wamra-n$_i$-ta$_j$ rika-ra-n* [*Tumas* [*e$_j$ maqa-yka-q-ta*]]

Based on these examples, we can formulate a tentative generalization:

> The *-n* of *wamra-n* preferentially refers to the subject of the clause in which it occurs.

In the sentences I asked people to judge, some were structurally ambiguous. For these, the respondents split fairly evenly over whether *wamra-n* referred to John's or Tom's son, and some

respondents indicated that it could be either. One such sentence is given in 12 and 13; these have the same terminal string, differing only in the structure and indices I have added:[5]

12) *Hwan*$_i$ [$_{VP}$ *rika-ra-n wamra-n*$_i$*-ta*$_j$ [*Tumas* e_j *maqa-yka-q -ta*]]

13) *Hwan* [$_{VP}$ *rika-ra-n* [*wamra-n*$_i$*-ta Tumas*$_i$ *maqa-yka-q-ta*]]

Consistent with our generalization, in these cases the reference of the *-n* of *wamran* depends on whether **John** or **Tom** is the subject of the clause in which *wamra-n* occurs. Another example follows:

14) *a. Hwan*$_i$ *wamra-n*$_i$*-ta*$_j$ [*Tumas* e_j *maqa-yka-q-ta*] *rika-ra-n*
b. Hwan [*wamra-n*$_i$*-ta Tumas*$_i$ *maqa-yka-q-ta*] *rika-ra-n*

Now let us consider a different case. When *Tumas* is the direct object of the matrix clause, *wamran* refers preferentially to Tom's son:[6]

15) *Hwan* [*Tumas*$_i$*-ta* [*rika-ra-n* [e_i [*wamra-n*$_i$ *-ta maqa-yka-q -ta*]]]]

We understand Example 15 as follows: The empty subject of the subordinate clause is co-indexed with the closest c-commanding noun phrase,[7] namely *Tumasta* in the higher clause.

Two things are worthy of note: First, this generalization—that *-n* is co-indexed with the closest c-commanding NP—also covers the previous cases, provided we assume that subjects c-command their objects.[8]

Second, in Example 15, the empty category in the subject position of the subordinate clause bridges the reference of *-n* (of *wamra-n*) to the object of the higher clause. This *-n* is still co-indexed with the subject of the clause in which it occurs, even though this is an empty category.

Similar examples follow:

16) *Hwan Tumas*$_i$*-ta* [e_i *wamra-n*$_i$*-ta maqa-yka-q-ta*] *rika-ra-n*

17) *Hwan rika-ra-n Tumas*$_i$*-ta* [e_i *wamra-n*$_i$*-ta maqa-yka-q-ta*]

18) *Tumas*$_i$*-ta rika-ra-n* [e_i *wamra-n*$_i$*-ta maqa-yka-q-ta*] *Hwan*

19) *Tumas$_i$-ta rika-ra-n Hwan.* [*e$_i$ wamra-n$_i$-ta maqa-yka-q-ta*]

In conclusion, provided we understand constituent structure and the inaudibilia within it, a simple generalization covers many (perhaps all) cases, namely, *-n* '3p' (as in *wamra-n* 'his son') refers to the closest c-commanding noun phrase.[9]

Condition B of the binding theory

We can learn another lesson from the data we have been considering. In all the examples above, if we replace *wamra-n* 'his son' by *pay-pa wamra-n* 'of him, his son', we force exactly the opposite co-reference. For example, compare 20a with 10 (repeated here as 20b):

20) a. [*Tumas$_i$ pay$_{j\neq i}$-pa wamra-n$_j$ ta maqa-yka-q-ta*] *rika-ra-n Hwan$_j$*
John$_j$ saw Tom$_i$ hitting his$_{j\neq i}$ son.

b. [*Tumas$_i$ wamra-n$_i$-ta maqa-yka-q-ta*] *rika-ra-n Hwan.*
John saw Tom$_i$ hitting his$_i$ son.

The explanation lies in a principle of universal grammar, namely Condition B of the binding theory:

> A pronominal cannot be bound (that is, co-indexed with a c-commanding phrase) in its governing category (that is, some limited domain, generally the minimal NP or S containing the pronoun and its governor).[10]

The governing category of *pay* in 20a is the entire subordinate clause. Since *Tumas* c-commands *pay* in that domain, *pay* cannot be co-indexed with *Tumas* without violating Condition B. Therefore, *pay* must refer to *Hwan* or to some other person.[11]

As further illustration of this principle, compare examples 21a and b:

21) a. *Hwan$_i$ warmi-n$_i$-ta kuyan.*
John wife-3p obj loves
John$_i$ loves his$_{i/j}$ wife.

b. *Hwan$_i$ pay$_{j\neq i}$ -pa warmi-n$_j$ -ta kuyan.*
John he gen wife-3p obj loves
John$_i$ loves his$_{j\neq i}$ wife.

The favored reading of 21a is that John loves his own wife, but it can also be interpreted as meaning that John loves someone else's wife. However, 21b can **only** be interpreted as meaning that John loves someone else's wife. This results from Condition B in the following way. For both examples, the governing category is the entire S. Since *Hwan* c-commands *pay* in that domain, *pay*, a pronoun, cannot be co-indexed with *Hwan*. Thus, *pay* must refer to someone other than *Hwan*, so *pay-pa wanmi-n* refers to someone else's wife.

Let us now reconsider Example 4. Recall that, from the perspective of proximity and order, we were surprised that the pronoun *pay* was used to refer back to *Josë*—the immediately preceding word—rather than a gap. That point at issue is represented by *j* in Example 22:

22) $[_S$ *Mana* e_j *perdunashqatsu Josë*$_j$*]* $[_{PP}[_S$ e_i $[_{S'}$ *pay*$_j$ *rantikushqan*]] *-ta*]
not did.not.forgive Joseph that.they.sold obj
Joseph$_j$ did not forgive them$_i$ for (their$_j$) having sold him$_j$

We are now in a position to understand why *pay* is appropriate at *j*. In Examples 15-19 we saw the possessor of the object co-indexed with an empty category in the position of the subject. Likewise, an empty category at *j* in 22 could be co-indexed with the subject e_i of that clause. But this would give the wrong result.

By contrast, the pronoun *pay* at *j* may not be co-indexed with the subject because it would then be bound in its governing category in violation of Condition B; *pay* thus forces us over the subject of the subordinate clause so that we can correctly identify *Josë* as the referent.

A principle of universal grammar nicely accounts for a number of Quechua cases for which we otherwise have no nice explanation. It is perhaps worth noting that, unlike the universals formulated in typological/functional linguistics, which usually deal with more superficial aspects of the language, this principle requires an understanding of structure and of notions like c-command, governor, governing category, and so forth. Although these may seem a bit complicated, grappling with them is well worth the effort.

Conclusion

Insights such as those discussed above convinced me that constituent structure, inaudibilia and the principles of modern grammatical theory are very enlightening and can lead us to a deeper understanding of a language than is otherwise possible. We are cheating ourselves out of a great deal by turning our back on this sort of linguistics.

Notes

1 These remarks were part of a colloquium I gave at the 1990 Summer Institute of Linguistics held at the University of Oregon.

2 For example, the gaps resulting from movements must be licensed by a very local sponsor, called a 'governor'. The reason is essentially functional: If it were otherwise, how would anyone ever know where they occurred?

3 The category label [S,NP] indicates a constituent that is internally like a sentence (with a substantivized verb) but which has the external distribution of a noun phrase. Traditionally *-ta* 'obj' has been regarded as simply a case marking suffix. However, I take it to be a post-position and—following Emonds' (1985) proposal to assimilating COMP and P—in cases like this I regard *-ta* as equally a COMP or a P. Therefore the following is an equally valid representation of the complement:

$[_S\ [_{S,NP}$ *Tumas wamra-n-ta maqa-yka-q*$]\ [_{COMP}$ *-ta*$]]$

One motivation for this perspective is that *-ta* demonstrates certain COMP-like behaviors; for example, it is an 'escape hatch' for movement.

4 The *j* subscript reflects an analysis whereby *wamra-n-ta* has moved from the lower to the higher clause, receiving its semantic role by virtue of the co-indexed 'trace' in the lower clause. This analysis is not entirely unproblematic since, at first blush, it seems that *wamra-n* also receives a semantic role from the verb of the higher clause. An alternative analysis would claim that *wamra-n-ta* is 'base generated' in the higher clause.

5 In this paper I will assume, perhaps somewhat simplistically, that *wamra-n* can be a member of the higher clause if it is adjacent to other elements of that clause. I will not be unduly concerned about its structural relationship to the higher clause.

6 When *Tumas* escapes the lower clause, it inherits a copy of the clause's case marker (perhaps as a reflection of having used that COMP as an escape hatch). We assume it is adjoined to the VP and that it does not, therefore, receive a semantic role from the verb of the higher clause.

7 A very informal characterization of 'c-command' follows: Imagine the path between the sentence structure's highest node and the node that dominates *y*. Then *x* c-commands *y* if *x* is connected—without any major intervening nodes—to some node on that path. Intuitively, this assures *x* a measure of prominence with respect to *y*.

8 This is unproblematic in most cases, but not when the subject comes between the object and the verb. There are various ways this might be handled, but considering these would take us too far afield for our present purpose.

9 There is only one apparent counterexample among the many possible ways to say 'John saw Tom hitting his son':

?Hwan$_i$ [[[e_i *wamra-n*$_i$*-ta maqa-yka-q-ta*] *nka-ra-n*] *Tumas*$_{*i,j}$ *-ta*]

It seems that the immediate precedence of *Hwan*—coupled with the great distance of *Tumasta*—encourages co-reference with *Hwan* rather than *Tumas*. I am not troubled by this case because it is probably not well-formed; speakers find it very strange. It seems to be a 'garden path' sentence: If it were to end right after *nkaran*, it would be a perfectly natural way to say 'John$_i$ saw him$_j$ hitting his$_i$ son'. The analysis would be as follows:

Hwan$_i$ *wamra-n*$_i$*-ta*$_k$ [[*pro*$_j$ e_k *maqa-yka-q-ta*] *nka-ra-n*]

When *Tumas-ta* is then encountered, it is most naturally interpreted as adjoined to the verb phrase:

Hwan $[_{1P}$ $[_{1P}$ *wamra-n-ta* [[*pro e maqa-yka-q-ta*] *nka-ra-n*]] *Tumasta*]

By the generalization that covers all the other cases, the *-n* of *wamran* should be co-indexed with the closest c-commanding NP, which would be *Tumasta*. Apparently, however, its co-indexation to *Hwan* is—by the time *Tumas-ta* is encountered—sufficiently entrenched to resist change.

10 Formal definitions of 'governing category' are more involved; the interested should consult a text such as Lasnik (1988). What is important here is the notion of a local domain in which certain conditions must be satisfied.

11 As an isolated sentence, *pay* naturally refers to *Hwan*; this is probably because it is the only other referent in this limited context.

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Fifth Biennial Symposium of the Department of Linguistics and Semiotics

Rice University
March 31 – April 3, 1993

I. Les Bruce

The theme for the conference this year was "Descriptive and Theoretical modes in the Alternative Linguistics". The participants presented the following papers:

Stephen Tyler (Rice)	Prolegomena to the next linguistics
Michael G. W. Bamberg (Clark)	What constitutes good data for the study of language development?
Philip W. Davis (Rice)	The way of language: Dimensions of VOICE
Barbara A. Fox (Colorado)	Towards a new methodology for functional syntax
John W. DuBois (UCSB)	Discourse and the ecology of grammar
Sandra A. Thompson and Tsuyoshi Ono (UCSB)	What can conversation tell us about syntax?
Paul J. Hopper (Carnegie-Mellon)	A discourse view of the verbal complex in English
Robert Van Valin (SUNY, Buffalo)	The interaction of pragmatics and syntax: A case study in restrictions on question formation, topicalization and relativization
Ronald W. Langacker (UCSD)	Viewing in cognition and grammar
John Haiman (Macalester)	Agreement and identity
Anna Wierzbicka (Australian National University)	Dictionaries and encyclopaedias: How to draw the line

The linguistics modes discussed at the meetings were functional or cognitive alternatives to approaches which postulate the autonomy of syntax, typically the current derivatives of Transformational Grammar. Issues addressed and explanations offered were varied. As has been the case for a long time, functional approaches still lack a common descriptive framework, although there is general agreement of some of the basic postulates of the movement. The cognitive or functional claims about language have been expressed

somewhat differently by different members of the group like Talmy Givón, Ronald Langacker, and Anna Wierzbicka. Langacker in *Concept, Image, and Symbol: The cognitive basis of grammar*, 1991, has summarized his views of language as follows:

> Language cannot be adequately described without reference to **cognitive** processing. Grammatical structures and patterns do not operate by grammatical rules which are independent of meaning (syntax is not autonomous). Grammar is a system of **symbolic** structures serving as a mode of expressing meaning (conceptual content) as structural **images** as much as the word in the lexicon is a mode of expressing meaning.
>
> Lexicon, morphology, and syntax form a continuum of symbolic units divided only arbitrarily into separate components; it is ultimately as pointless to analyze grammatical units without reference to their semantic value as to write a dictionary which omits the meanings of its lexical items... Moreover, a formal semantics based on truth conditions is deemed inadequate for describing the meaning of linguistic expressions (Langacker, 1991:1).

Despite the diversity of the group at the conference, the participants grappled with some common issues:

> Robert Van Valin responded to the challenge of offering a better rigorous analysis of grammar than autonomous syntax approaches in his paper on 'The interaction of pragmatics and syntax'. Although everyone agreed that grammar was not in essence autonomous, most participants admitted to some arbitrariness in grammar and tried to address the question of how to fit in a notion of grammaticalization in a functional approach to grammar.
>
> Wierzbicka's paper was unique in dealing exclusively with the lexicon, but the issue of the correct approach to semantic description applies to both lexicon and grammar. In admitting cognition into linguistic descriptions, can one draw a line between linguistic meaning and all of the experience speakers bring to the speech event (pragmatic processes and knowledge)?
>
> Anna argued that such a line can be drawn on linguistic grounds. Most, if not all, of the other participants either did not agree that such a line could be drawn, or they presented no suggestions on how to distinguish grammatical explanations from other factors which impinge on the use of language. Thus the distinction between a language speaker's **competence** and his **performance** is completely erased.
>
> Thompson and Ono's paper stretched the limits of grammatical description by tackling unedited conversation. Their discussion

involved cultural patterns of conversational activity, false starts and situational interference.

Papers by DuBois and Hopper were restricted to discourse-level functional explanations of clausal syntax.

A conference like this typically provides opportunities for personal interaction. It was re-energizing to discuss Papuan linguistics with John Haiman and Robert Van Valin.

The most disappointing part of the trip was that there was time for little more than an exchange of greetings with Anna Wierzbicka since she arrived to present her paper only after lunch on the last day of the conference . Her work on lexical semantics is particularly important for SIL field workers. She has had input to the organization through her students—myself included. I hope we can give her a broader platform to share with our membership her theory and methodologies for doing careful semantic analysis. ■

Fifth Biennial Symposium of the Department of Linguistics and Semiotics

Rice University
March 31 – April 3, 1993

II. Charlie Law

This year's installment of the symposium series was billed as a discussion of 'descriptive and theoretical modes in the alternative linguistics'. I wanted to go because Paul Hopper, Sandy Thompson, Robert Van Valin and Ronald Langacker were all on the program. I'd begun to run across their names a lot as I've read in preparation to teach Grammar II in the strat model.

'Alternative' is a good description of what we heard at Rice. Alternative to what? The mainstream chomskyian tradition of formal academic linguistics, I suppose. 'We decided it was time for us to take back syntax,' Sandy Thompson said in introducing her work on conversation analysis. '@#$%, let's take back linguistics!" Philip

Davis of Rice responded. The politics of linguistics came up a few more times during the conference, but I didn't sense that these alternative linguists see themselves as an oppressed minority. Instead I think they're starting to suspect that they've been right all along, after all.

Among the things these folks are currently studying are the syntax (or near-syntax) of conversation; why English speakers in real discourse prefer strung-out verbal complexes to simple verb forms selected from a paradigm (e.g., 'I kept on going straight ahead' rather than 'I continue straight'); and a novel idea called 'the ecology of grammar'—grammar as adaptive to its semantic and pragmatic environment. The presentation which most resembled formal linguistics was Robert Van Valin's talk on the interaction of pragmatics and syntax in question formation, topicalization and relativization; I take it he sees himself as a hybrid.

The panel's closing statements offered a glimpse of where linguistics—or at least this brand of it—may be going. 'Let's face it: linguistics isn't a science,' John Haiman declared. 'It's a **humanity**.' 'We're reaching the point where perhaps the best we can do is hope to make principled commentary,' mused Paul Hopper. This is a long, long way from the expressed objective of chomskyian linguistics: not just to describe but to explain, and that with nearly mathematical precision. I get the feeling that Hopper, Haiman, Thompson, *et al.*, freed for some time now of the constraints of sentence grammar, have seen just how much is out there in the world of real language, and how complicated it all is, and how piddling is our current grasp of it.

These comments were not discouraging to me. I noticed a while back that I come up against inexplicable linguistic phenomena with increasing, not decreasing, regularity. So it was encouraging to hear the experts saying essentially the same thing. ■

REVIEWS OF BOOKS

Conversational competence and social development. By Ioanna Dimitracopoulou. Cambridge: Cambridge University Press, 1990. Pp. 208. Hard cover $44.50

Reviewed by Martha Harris

Dimitracopoulou claims:

> Most pragmatists working in the area of child language assume an interdependence of language and social functioning...
>
> Nevertheless, there are few studies which attempt to examine in detail the interdependence of language and social functioning (p. 41).

In this book she successfully provides a promising start to fill in this gap.

Conversational competence and social development focuses on the development of children's language and how this relates to their social development. Dimitracopoulou states that her general aim is to find out 'what children can and cannot do with language at different periods in their development' (p. 27). She reaches this goal by discussing the research that has been done by others in this area and relating it to her own detailed research on the subject.

There are seven chapters in the book. Chapter 1 introduces the pragmatic study of language, which focuses on the function and social context of language. It also gives a general outline of the rest of the book.

Chapter 2 focuses on the development of speech-act theory and on the characteristics of conversation. Chapter 3 goes on to review the research that has been done in these two areas and then examines the relationship between language and social functioning. Dimitracopoulou gives a very thorough discussion in these two chapters. This gives a good context for understanding the methodology of her research.

Chapters 4-6 present the author's own empirical research findings concerning linguistic development, including a detailed description of

the methods she used and an analysis of the results. Dimitracopoulou used 19 children, ages 3½ to 7, in her study. She selected them from four age groups: ages 3½ to 4, 4½ to 5, 5½ to 6, and 6½ to 7. Each subject first participated in a game (which was videotaped) with his or her own mother. Immediately following the game, the children participated in three role-taking tasks with the experimenter.

Transcriptions of the game situation were analyzed using Bach and Harnish's model of speech-acts, which classifies utterances into six major categories: assertives, responsives, requests, commissives, expressives, and acknowledgment. A socio-interactional analysis of the transcriptions was also performed.

The three role-taking tasks were designed to measure the children's abilities to understand the perspective of other people, and the ability to understand social contexts and conventions. The tasks included (1) a face sensitivity task, (2) a visual-perspective-taking task, and (3) a social-event-reporting task. The relationship between the children's performance on these tasks and their linguistic performance during the game was examined in detail, with the results suggesting 'a strong positive correlation between the two' (p. 130). This provides clear evidence that language use and social functioning are interdependent, the author says.

Dimitracopoulou admits that one of the limitations of her study was the small number of subjects. She suggests that further research should seek to test a larger number of children from more varied backgrounds, but for her own purposes 'a detailed analysis of a relatively small sample of the children's language use is preferable to a more superficial analysis of a larger sample...' (p. 53).

The book concludes with chapter 7 giving a summary of the findings, implications for theorizing about language, and applications for language learning and instructing. The applications of the author's findings (given in the last section) are related to the concerns of parents, teachers, and therapists. Although I found the book to be written in an advanced, difficult style, the author gives a careful description of the social and linguistic issues underlying communication. If she is correct, a child's active participation in conversation is critical for the development of language and many language problems may stem from specific social problems. ■

Relevance relations in discourse: A study with special reference to Sissala. By Regina Blass. Cambridge: Cambridge University Press. 1990. Pp. 300. $49.50

Reviewed by E. Lou Hohulin

In my living room, tucked into the corner of a bamboo chair, is a small, cross-stitched pillow. There is a picture of a smiling, green frog with a caption that reads, 'You'd be happy, too, if you could eat what bugs you.' I wonder if Regina Blass, the author of the book which I am reviewing, might have had feelings akin to those of the smiling, green frog when she completed her research on a selected set of pesky particles in Sissala, a language spoken in Burkina-Faso and Ghana.

This book, *Relevance relations in discourse: A study with special reference to Sissala*, was developed and written on the basis of Blass' doctoral thesis, and its main topic is pragmatic connectivity in discourse. In her introduction to the book, she says that her main intention in writing the book is 'to show, using Sperber and Wilson's relevance theory...that connectivity in discourse is a pragmatic rather than a semantic matter: it results from relevance relations between text and context rather than from relations linguistically encoded in the text' (p. 1). The main empirical evidence which Blass uses for proving her claims are thirteen Sissala forms which she calls particles. Blass' corpus of data for studying these particles was 2,000 pages of natural language text.

In the first chapter, Blass discusses selected approaches to the analysis of discourse and chooses, in particular, to focus attention on methodology in analysis, and the manner in which these approaches handle notions of cohesion, coherence, topic, and context. In the final section of this chapter, she summarizes her conclusions: 1) 'discourse is not a purely linguistic notion', and 2) 'cohesion, coherence and topicality are neither necessary nor sufficient for textuality and comprehension'. One of Blass' goals in her research was to propose a framework for data-based discourse analysis so she says that the main point of Chapter 1 'has been to show what this framework should not be based on' (p. 41).

Blass argues against classic accounts of cohesion, coherence, and topic in discourse structure on the basis that underlying them is a semiotic approach which she believes is inadequate to account for verbal communication (p. 42). In her view, the key to textuality is 'something deeper' than what can be accounted for by the analysis of text structure. This 'something deeper' is identified as the relation between text and context (p. 19-20). Her framework for studying this relationship, called discourse connectivity, is based on her adoption of 'the suggestion of Sperber and Wilson (1986a) that what is crucial to discourse comprehension is the recognition of relevance relations, which are relations between the content of an utterance and its context' (p. 24-25). It is important to note that Sperber and Wilson are only claiming this notion as being crucially important to discourse comprehension. Blass, on the other hand, has chosen to make the notion a part of a framework for discourse analysis.

Another important thing to note is that Blass' definition of context, based on Sperber and Wilson, is very different from the definition of others who study discourse. She states 'By **context** here I do not mean the real world or the co-text (the preceding or following text of a discourse); rather it is a set of assumptions retrieved or derived from memory or acquired by perception, and used in the interpretation process' (p. 9). Her definition of context is one of the most critical notions for the reader to keep in mind as the book is read. There can be no clear understanding of her claims and arguments without relating them to that particular notion of context.

In Chapter 2, Blass presents the aspects of relevance theory which relate to her research. She discusses relevance theory and comprehension, relevance theory and pragmatics, relevance theory vs. coherence theory, and relevance theory and textuality. At the end of the chapter, she states that there are two fundamental assumptions of relevance theory that have played an important role in her research. The first assumption is that the linguistic form of an utterance grossly under-determines its interpretation and the second is that the gap between what is linguistically encoded and what is communicated is filled by an inference process which is constrained by the criterion of consistency with the principles of relevance.

Those two assumptions are fundamental to the arguments she gives against the discourse analysis approaches discussed in Chapter 1. They also underlie her disagreement with researchers who view the

notion of 'understanding discourse' (i.e. notions of comprehension and interpretation) as something very different from the notion of 'analyzing discourse' (i.e. analyzing the linguistic structure of text). She states:

> I am not suggesting that the linguistic structure of the text should be ignored... However, to me, discourse analysis is nothing else but tracing the hearer's part in understanding utterances, and I claim that any other approach either yields uninteresting statements of statistical frequency, or is like going on a journey without a destination in mind (p. 11).

And so it appears that utterance interpretation is the essential core of the discourse analysis framework which Blass is proposing.

The remaining chapters of the book, Chapters 3-8, are Blass' presentation and interpretation of Sissala data. These chapters are, in her words, 'dealing with more specific issues concerning the relation between linguistic form and pragmatic interpretation' (p. 92), and these data are intended to be her evidence for supporting her claims about the necessity for dealing with utterance interpretation as the key for opening the door to an adequate account of textuality.

'Hearsay' particles and the Sissala interpretive-use marker

In Chapter 3, Blass defines a minimal hypothesis for explaining the use of 'hearsay' particles. She says:

> What is the minimal hypothesis one might make about hearsay particles, given only the informal observation...that hearsay particles are used to mark information that the speaker got from somebody else? The minimal hypothesis would be, I think, that they should be used only for reporting actual speech (p. 93).

Blass, then, discusses two main accounts of the nature and function of 'hearsay' particles. One account is that 'hearsay' particles have a modal or evidential function. Blass cites Palmer (1986) as the main source for this account. She says:

> He sees the main function of hearsay particles in terms of the intention of the speaker to express his degree of commitment to the information being conveyed....(and) falling into the same semantic class as 'may' and 'might'... According to Palmer (1986:53), there are two different systems of epistemic modality found in the languages of

> the world: judgments and evidentials. Judgments involve speculation and deduction; evidentials include perceptives and quotative (hearsay) devices (e.g. particles and morphological marking) (p. 93-95).

Blass continues by saying:

> This fits well with the widespread view that there are three types of propositions expressible in language. For example, Givón (1982:24) proposes the following classification:
>
> 1 Propositions which are taken for granted, via the force of diverse conventions, as unchallengeable by the hearer and thus requiring no evidentiary justifications by the speaker.
>
> 2 Propositions that are asserted with relative confidence, are open to challenge by the hearer and thus require—or admit —evidentiary justification.
>
> 3 Propositions that are asserted with doubt as hypotheses and are thus beneath both challenge and evidentiary substantiation (p. 95).

The alternative account, which Blass chooses as her model for explaining the Sissala 'hearsay' particle, is that of Sperber and Wilson (1986a), who make a distinction between descriptive and interpretive uses of language and thought. Blass describes this distinction in these words:

> An utterance or assumption can be descriptively used to represent a state of affairs—that state of affairs which would make it true. Or it can be interpretively used to represent another utterance or thought which resembles it.

She goes on to say that based on this distinction, 'hearsay' particles function as grammatical indicators of interpretive use (p. 101). Her evidence for this claim is the Sissala particle *ré* which she understands to be a grammaticalization of a certain mode of representation, that of interpretive use.

Blass' objective in this chapter is to disprove her minimal hypothesis that a 'hearsay' particle may only be used for reporting actual speech, and she accomplishes that objective. Nevertheless, it is not clear how her analysis and explanation of the Sissala data refutes the Palmer and Givón approaches. According to her, the main evidence for the interpretive analysis (non-modal) was the following:

> The use of *ré* with irony; the use of *ré* under verbs of propositional attitude, such as belief and desire; and the use of *ré* in questions and answers to questions (p. 100).

I compared the Sissala 'hearsay' particle data with data on the 'hearsay' particle in Tuwali Ifugao, an Austronesian, Philippine-type language. The two particles are very unlike in usage. None of Blass' three criteria for the interpretive analysis as stated above is true of the Tuwali 'hearsay' particle, *kanu*. The conventionalized function of *kanu* is to indicate that the information stated is reported, i.e. a paraphrase of what someone else has said. It expresses evidential modality and is not obligatory except when a statement might be considered challengeable. In context, linguistic and extra-linguistic, it is disambiguated as to whether the speaker wants to:

1 support his statement by indicating that others have believed and said the same thing;

2 indicate that the speaker recognizes that his hearers know that he was not an observer of the events that he is reporting or does not have the knowledge or expertise to make such statements.

For the Tuwali data, the Palmer-Givón-type account is preferable to the interpretive account chosen for the Sissala particle.

I have chosen to make more extensive comments on this chapter than any other because Blass used one example from a Philippine language as evidence for her claim about the use of the Sissala 'hearsay' particle as an interpretive marker for irony. On the basis of this example, Blass made the following claims:

> In languages where interpretive use is grammaticalized, and where its use is obligatory, irony will have to be expressed with an overt marker. This is obviously the case in Philippine languages (p. 109).

Since none of these claims is true of the Tuwali Ifugao 'hearsay' particle, I want to briefly discuss the example which she cited.

The example is taken from an article by Lee Ballard (1974). In his article he stated that the 'hearsay' particle in Philippine languages seems to be used universally to report speech. Then, he gives one example of the use of the Ibaloi 'hearsay' particle, *kono*, in the translation of Matthew 27:42 and states that he considers this example to be an 'odd sidelight', in that it is used to imply sarcasm.

> He saved others *kono*, but he cannot save himself. He is *kono* the king of Israel, so let him come down from the cross.

I believe that the 'hearsay' particle as it is used here does not explicitly mark sarcasm (or irony) and can only be interpreted to implicitly express it within the linguistic context which immediately precedes its use, i.e. v. 41 (NIV).

> In the same way the chief priests, the teachers of the law and the elders **mocked** him.

To sum up my discussion of this chapter, I do not intend to imply that a 'hearsay' particle cannot qualify as an 'interpretive marker' or that making a distinction between descriptive and interpretive use might not be a useful idea, but Blass' arguments and evidence have not convinced me that the criteria she cites for the Sissala particle is necessarily applicable to the 'hearsay' particles of other languages.

Particles, contextual effects and typology

In Chapter 4, Blass raises the question:

> If it is true, as was suggested in chapter 1, that all humans have the same logical abilities, and that communication creates a presumption of adequate contextual effects for the minimum justifiable effort, should we not expect to find in every language similar linguistic phenomena which save the hearer processing effort by guiding him towards the intended range of contextual effects? In other words, could the very fact that humans are constrained by considerations of relevance be the basis for a typology of non-truth-conditional particle phenomena (p. 124)?

Relevance theory, according to Blass, proposes three types of contextual effects: 1) contextual implication, 2) strengthening of assumptions or 3) contradiction of the assumptions of the hearers. All of these contextual effects relate to the inferences drawn by the hearer in a communication situation. In her discussion of contextual effects as they relate to particles, Blass first presents non-truth-conditional particle phenomena related to the views of Grice, Karttunen and Peters, and Blakemore. Then she compares Blakemore's work on inferential constraints on English lexical units to certain inferential devices in Sissala. She demonstrates her claims with the English forms, **after all, well, so, therefore**, and **also**, Sissala

particles, ***sié*** and ***má***, and German ***auch*** (p. 130-134). Her data and discussion range about the hypothesis that certain forms in languages are inferential devices which relate to contextual effects, providing ease of processing on the part of the hearers. On the basis of her comparison of the particles of the three languages, she concludes that there are cross-linguistic similarities among such inferential devices (p. 159).

Particles and truth conditions

In Chapter 5, Blass asks two questions, '...how does one decide whether a given linguistic expression is truth-conditional or not?' and if there is ambiguity, 'must they be analyzed as genuinely ambiguous between truth-conditional senses, or can they be treated as merely vague?' (p. 161). She discusses the Sissala particle *baa* and demonstrates with data that the particle has a number of different temporal and non-temporal interpretations which are ambiguous regarding truth-conditional and non-truth-conditional use, and also vague regarding different truth-conditional uses. She suggests:

> ...that truth-conditional phenomena may become non-truth-conditional via an intermediate stage where they modify implicit predicates supplied by pragmatic enrichment... the notion of pragmatic enrichment derived from relevance theory might enable us to achieve much simpler analyses on the purely linguistic level, which, when combined with content, context and considerations of relevance, might be flexible enough to deal with the rich diversity of data which analyses based on multiple ambiguity must necessarily ignore (p. 182).

Referential marking

Blass deals with certain Sissala referential forms in Chapter 6. To introduce the topic, she says:

> Every language with referential expressions seems to have some means of indicating to hearers not only that a referent of a certain type is required, but also where to search for appropriate referents, and how easily accessible they are. Within the framework of relevance theory, such expressions, in at least some of their uses, should be analyzable as semantic constraints on relevance contributing not to truth-conditional content but to reducing the hearer's processing load (p. 183).

To illustrate semantic constraints on relevance in this sense, Blass describes her analysis of three different Sissala defining particles: *ré*, *né* and *ná*. She explains that the typicality marker *ré* is a constraint on relevance or processing aid, guiding the hearer to access encyclopedic entries with stereotypical or schematic information. On the other hand, she describes the use of *né* as encouraging the hearer to open a new specific address and says that the definite marker, *ná*, is used with known or easily predictable concepts. In this chapter Blass has attempted to demonstrate that the relevance theory framework explains these markers better than a framework which assumes interpretation of references in discourse based on the mutual or shared knowledge of speaker and hearer.

Universal quantification

In Chapter 7, Blass argues against a purely predicate-calculus treatment of truth-conditional quantificational phenomena. She argues on the basis of her analysis of Sissala *wuu* which can mean **all, every, each, any, whole, very** and **always**. She discusses the ambiguity between truth-conditional and non-truth-conditional uses, and vagueness between different truth-conditional uses of this form.

She uses her data to demonstrate how a language can choose one form, with one semantic meaning, and then enrich that form so that it has a multitude of more specific meanings. According to her, this is done for pragmatic reasons, in particular for reasons of relevance. She chooses to account for her *wuu* data in terms of vagueness between different truth-conditional meanings which she believes provides simpler and more explanatory solutions than an account in terms of ambiguity or polysemy (p. 214).

Blass closes this chapter by saying:

> This section has shown how generic determination is related to universal quantification, but is not identical to it, and how the speaker may exploit the relation between the two in discourse. From a purely 'surface' point of view, the change from generics to non-generics seems odd. However, the hearer knows that these various representations are not just arbitrary, but are intended to guide him towards different contextual effects (p. 237).

Coordination and stylistic effects

In Chapter 8, Blass describes three Sissala forms, *ní*, *a* and *ká* which are equated to English **and**. She says that the use of these forms:

> ...is syntactically conditioned: *ká* is used to conjoin Ss, *a* is used to conjoin VPs, and *ní*, or *aní*, is used elsewhere. The different coordination constructions also differ in their pragmatic effects: for example, sentential coordination with *ká* might be analyzed as suggesting that the event described in the second conjunct was unexpected, whereas non-sentential coordinations carry connotations of stereotypicality (p. 238).

She concludes this chapter by saying:

> In this chapter I have tried to show that the implications of 'unexpectedness' that attach to certain cases of sentential coordination in Sissala do not arise from a lexical source, but receive a natural explanation within the framework of relevance theory (p. 258).

Research methodology

As I read and studied this book in order to write this review, I was perplexed by the fact that Blass does not address the issues related to the fact that she and Sperber and Wilson, the developers of relevance theory, use different methodologies in their research work. Sperber and Wilson use contrived sentences and introspection whereas Blass' primary data is from Sissala natural language texts, and since she is not a native speaker of Sissala she would have been limited in her use of introspection.

In fact, her only allusion to such methodological issues is when she says regarding her research:

> Some may say that a speaker from one culture operating with data from a very different culture will meet insuperable problems... Communication occurs in a social and cultural context, which strongly influences people's assumptions about the world. The discourse analyst has to know some of the hearer's assumptions, no matter in what culture the analysis is done. It is necessary, therefore, for a discourse analyst in a quite different culture to live in that culture and work closely with native speakers. In the case of the Sissala data, this was the situation.

There is no explanation of the procedures she used to determine a 'hearer's assumptions'; notwithstanding the difficulty of the situation, she proceeds on the basis of a stated assumption about 'universal logic',

> My assumption has been that people from all cultures operate with the same logic (p. 13).

Years ago, Labov (1971) wrote a classic paper on research methods which I think merits a quotation since it relates to this discussion. In his introduction, he says:

> There are two distinct but overlapping concerns that motivate the study of research methods. One is the desire to find an approved and practical procedure for gathering, processing and reporting data. The other is the need to discover if such results are right or wrong: to find ways of estimating the degree of error, isolate the sources of error, and eliminate them. The first approach involves tests of reliability: to be sure that different investigators will produce the same data and the same analysis from a given input. The second approach is concerned more with validity: notions of right and wrong imply some connection with a measurable or predictable empirical basis independent of the investigators or their school. Valid reports or theories must be shown to fit that secular reality... the term valid, is not current in linguistics... some linguists have argued that there is no uniquely correct analysis of linguistic data and imply the absence of any decisive evidence independent of the activity of the linguist... a concept of the validity of linguistic theories will be needed. It is proposed that a valid linguistic analysis will fit the characteristics of the language used in everyday life when the linguist is not present (pp. 413-415).

Stubbs (1983), too, is concerned about methodology and the reliability and validity of data in research. In one section of his book he has aptly pointed out that language forms and structure belong to a static system and communication belongs to a dynamic system and we do not yet have the necessary means to correlate them adequately. In applying the Labov and Stubbs concerns to the question of differences in the Sperber-Wilson and Blass methodologies, it has seemed to me that a linguist who uses a contrived sentence-introspection methodology studies the static system of a language and the competence side of linguistic study, whereas a linguist who analyzes natural language discourse studies the dynamic system of a language and the performance side of linguistic study.

Given the importance of research methodologies and the effect any given methodology can have on the analysis and interpretation of data, Blass' book would have been vastly improved if she had addressed these issues and given a thorough explanation of her compilation and processing of the text data.

Argumentation

The fact that Blass did not address methodological issues is, I believe, partially responsible for what I found to be a lack of coherence in her argumentation; it takes concentrated thinking to determine when Blass is addressing the basic premises of a particular theoretical approach to the study of language, when she is addressing the problems of developing an adequate theoretical framework for analysis, and when she is arguing for the best description and explanation of data.

Another coherence problem with the text is that she does not give a consistently clear presentation of the positions of linguists from whom she quotes. She simply presents certain notions with which she agrees or disagrees and one sometimes feels that she has not studied these notions sufficiently within the frameworks in which they were proposed. I was particularly bewildered by the fact that most of her argumentation against a linguistic approach to the study of discourse was confined to the first chapter and was largely unrelated to the presentation and interpretation of Sissala data in the later chapters. This lack of correlation between the first chapter and the later chapters inevitably weakens her argumentation throughout the book and keeps the reader from adequately grasping the new framework for discourse analysis which she is proposing.

Conclusions

The greatest strength of this book is the originality of the research. Blass has made a remarkable attempt to deal with some notoriously difficult data in a creative and interesting way. Also she raises important questions that must be considered whether or not one agrees with her particular answers. There is, no doubt, merit in the proposal that some answers might be found in consideration of the

notion of connectivity between text and context in the relevance theory framework.

One perspective presented in this book that deserves both consideration and further research by those of us involved in linguistic-translation field projects is Blass' proposed new goal in discourse analysis:

> ...tracing the hearer's route in the interpretation of the speaker's intention, and finding out how a piece of text can modify the hearer's assumptions about the world (p. 12).

This is what we translators attempt to do during comprehension checks of translated materials. We have a step-by-step procedure, and we know how to use it in order to improve the quality of our translations. Unfortunately, we have not developed a methodology for compiling and analyzing the data we collect during these comprehension checks. Perhaps Blass' work and that of others working in relevance theory can offer insights for those who might be challenged to develop such a heuristic.

Finally, this book is not a book for field workers who are looking for a definitive methodology or a step-by-step procedure for analysis of discourse, or even of particles. It is a book for those who are stimulated by thinking through methodology and linguistic argumentation, and enjoy attempting to refute claims. It is also a book that gives a provocatively different view of discourse study.

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Color and color perception: A study in anthropocentric realism. By David R. Hilbert. Stanford, CA: Center for the Study of Language and Information, 1987. Pp. ix, 146. Hardcover $24.95; Paper $11.95

Reviewed by John M. Clifton

Papua New Guinea

Quick now—do adjectives describe qualities in the 'real world' or are they entirely 'in the mind of the beholder'? The popular conception of an adjective like **straight** is probably that it describes an objective quality of an object. On the other hand, the popular conception of an adjective like **blue** is probably that it describes a subjective quality. What you call blue is not necessarily what I call blue. Furthermore, it is commonly believed that color terms describe perceptions, not an innate quality of the object observed, and so an object will 'change color' under different lighting conditions.

Recent studies have questioned these popular conceptions. Berlin and Kay (1969) and subsequent studies have shown that color terms do not vary arbitrarily between people and languages, while work in cognitive linguistics, as represented by Lakoff (1987), has proposed that even the most objective concepts are grounded in experience.

In this context, Hilbert's book is most interesting. The essential argument of the book is summarized by the statement that 'The nature of our perceptual experience determines which color categories we perceive but these categories are themselves independent of our perceptual experience' (p. 119). The primary thrust of Hilbert's work is to show that color is a property of an object, not of perception. In this sense Hilbert is a realist; he is

committed to the position that there is a real world and that we can have an accurate perception of that world. Color perception, however, is based in human experience even though it is a reflection of a real world. Further, Hilbert claims that even 'objective' qualities such as **straight** are inextricably based in human experience. The interaction of human experience with a real world form the basis for Hilbert's conception of 'anthropocentric realism'.

Much of the book is devoted to examining various objections to the claim that color terminology can be objective. Hilbert argues that these objections do not apply to 'anthropocentric realism'. At the same time, Hilbert discusses the physiological basis of color perception in more detail than other authors I have read, proposing that the objective basis of color is the spectral reflectance of an object.

Although the book is published by CSLI (Center for the Study of Language and Information), it deals only tangentially with the linguistic aspect of color terminology. However, its elaboration of the role of human experience in color perception makes it a welcome addition to recent studies in cognitive linguistics.

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Coordinator's Corner

Progress in Tools For Semantic Classification

Les Bruce has come to a turning point in the development of the first stage of producing **Tools for Semantic Classification**. Les is undertaking to develop a thesaural help to enable you to semi-automate the classification of each lexical entry in your dictionary. The goal: better semantic analysis and cross language comparisons by providing you with help to **contrast words with semantically similar lexemes in one language with those of another language** by arranging sets of related words in the vernacular lexical data base and producing a vernacular thesaurus. The semantic domains are taken from Louw and Nida's Greek-English lexicon to make comparisons with the Greek a snap.

Les says:

> Our team has completed a test version of an English thesaurus in database format. It can be used now in FIESTA (FIESTA data base is available). You simply search for the words of an English gloss or definition of your dictionary entry to find the semantic domain that the vernacular word belongs in. To make this work we need the English thesaurus with its synonyms listed in semantic domains to match with the words of your definitions.

Part of the package includes two hierarchies of semantic domains. One hierarchy lists Louw and Nida's major and minor domains. A five-level hierarchy enhances the translator's search for semantic domains from the top down, through the hierarchy. That hierarchy is designed for use in the outliner view in Winword and Word for DOS.

Les would love to hear from others working on thesauri, and is interested in having others test what he has come up with. For more information send inquiries to Les Bruce at the Dallas center.

—Eugene Loos

Implicatures

Ernest W. Lee

A question that comes to mind when one first encounters the word *implicatures* is how does the term relate to what is commonly referred to as implied information and more specifically, what contribution does implicatures have to translation principles. There is a relation but not a one-to-one correlation between *implicatures* and *implied information*; this will be more apparent in the subsections on various implicatures. The usefulness of implicatures for translation goes beyond what is usually included under the rubric implied information.

This is a preliminary study in preparation for a section on implicatures in the Linguistics Field Manual for Project '95. As such, feedback concerning how the information could be made clearer or could be made more useful to the field worker would be appreciated.

Levinson's comments on the importance of the notion of implicature provide a good starting point as an introduction to implicatures:

> First, implicature stands as a paradigmatic example of the nature and power of pragmatic explanations of linguistic phenomena...
>
> A second important contribution... is that it provides some explicit account of how it is possible to mean (in some general sense) more than what is actually *said* (i.e. more than what is literally expressed by the conventional sense of the linguistic expressions uttered)...
>
> Thirdly, the notion... seems likely to effect substantial simplifications in both the structure and the content of semantic descriptions...
>
> Fourthly, implicature or at least some closely related concept, seems to be simply essential if various basic facts about language are to be accounted for properly...
>
> Finally, the principles that generate implicatures have a very general explanatory power: a few basic principles provide explanations for a large array of apparently unrelated facts (1983:97-100).

The following example comes from H. P. Grice, who first introduced the notion of implicatures:

> Suppose that A and B are talking about a mutual friend, C, who is now working in a bank. A asks B how C is getting on in his job, and B replies, 'Oh quite well, I think; he likes his colleagues, and he hasn't been to prison yet.' At this point, A might well inquire what B was implying, what he was suggesting, or even what he meant by saying that C had not yet been to prison. The answer might be any one of such things as that C is the sort of person likely to yield to the temptation provided by his occupation, that C's colleagues are really very unpleasant and treacherous people, and so forth. It might, of course, be quite unnecessary for A to make such an inquiry of B, the answer to it being, in the context, clear in advance. It is clear that whatever B implied, suggested, meant in this example, is distinct from what B said, which was simply that C had not been to prison yet. I wish to introduce... the verb *implicate* and the related nouns *implicature* (cf. *implying*) and *implicatum* (cf. 'what is implied') (1975 in Davis, 1991:306).

Grice coined the term *implicature*, then, to label the portion of meaning conveyed or intended to be conveyed, which is not part of what was actually said. This defines implicature negatively since it is what is left over after what is said has been removed. Concerning this, Sadock says:

> It is often the case that negatively defined classes are not uniform classes; their only common defining property is that none of the members of the class has one particular property. I suspect this is also true of Grice's class of implicatures (1975 in Davis, 1991:365).

Implicatures are generally treated as either *conventional* or *conversational*. The conventional 'include all nontruth-conditional aspects of what is conveyed by an utterance solely due to the words or forms the sentence contains' (Sadock, 1978 in Davis, 1991:366). Levinson (1983:129) gives examples among which are the honorifics prevalent in languages of South East Asia and the *tu/vous* distinction of French, etc. There is no difference in the truth-conditions of *Tu es le professeur* and *Vous etes le professeur*, except in the social relationship (close versus distant) of speaker and hearer. The difference is conveyed solely in the words *tu* and *vous* and the attendant verb inflections. Not all scholars accept the notion of conventional implicatures; some attempt to handle the same phenomena in other ways. Conversational implicatures are those not conveyed solely due to the words or forms contained in the sentence. The illustration of the dialogue about the man working in the bank is a conversational implicature.

In much of the literature the term *implicature* is used to refer only to conversational implicatures. Because most of the interest and fruitfulness of the study of implicatures relates to the conversational group, I attempt to keep the references to conversational and conventional implicatures separate. Conversational implicatures are treated first and the conventional ones later.

Conversational Implicatures.

In conversation, speakers and hearers expect that speakers will, as much as possible, abide by certain principles which will make it possible for the hearers to determine the full meaning of what each speaker intended to convey. To grasp the meaning of conversational implicature, it is essential to be familiar with these underlying principles. There is only one general principle in conversation which leads to four subprinciples or submaxims—the **Cooperative Principle**:

> Make your contribution such as is required, at the stage at which it occurs, by the accepted purpose or direction of the talk exchange in which you are engaged (Grice 1968, in Davis, 1991:307).

This assumes that a speaker is being cooperative so that he does what he can to communicate his intentions to his addressee. Negatively, it assumes that he does not deliberately seek to miscommunicate.

The four submaxims are those of Quantity, Quality, Relation, and Manner. (Grice derives these categories from Kant.) Some of the maxims he considers to be of greater importance than others.

1. **Quantity** [information provided]
   a. Make your contribution as informative as is required (for the current purposes of the exchange).
   b. Do not make your contribution more informative than is required. [This maxim may itself be overinformative since it is probably adequately covered by the maxim of relevance. Extraneous information may be misleading since the hearer may think there is some relevance to it when there isn't.]

2. **Quality** [the truthfulness of the information provided]
   a. Do not say what you believe to be false.
   b. Do not say that for which you lack adequate evidence.

3. **Relation** [the relevance of the information]
   Be relevant.

4. **Manner** [not *what* information is provided, but *how* it's provided]
   a. Avoid obscurity of expression.
   b. Avoid ambiguity.
   c. Be brief (avoid unnecessary prolixity [wordiness]).
   d. Be orderly.

Conversational implicatures can be the result of either observing the above maxims or by violating them. For implicatures of the first type Levinson (1983:104) introduces the term *standard implicature* since Grice does not have a cover term for these. The implicatures which arise through violating the maxims are referred to by Grice (1975, in Davis, 1991:310) as *floutings* or *exploitations*. They have also been called *breaches* and *infringements* of the maxims. The term *flout* and its derivatives seem to be most widely used and although I do not like them, I will use them and refer to the derived implicatures as *flouting implicatures* in contrast to *standard implicatures.*

The following sentence illustrates an implicature which is the result of observing the maxim of Quantity:

Noah had three sons. (Genesis 6:10)

The implicature which derives from this sentence is that Noah had no more and no less than three sons. In this particular case it is confirmed by immediately listing the three sons. The statement, however, also entails that Noah had one son and that he had two sons since both of those would also be true. The notion of *entailment* is frequently encountered in discussions of implicatures; anything less than or weaker than the full import of an implicature is an entailment, that is, it is included in the implicature. Other examples are: **I know** entails **I think**, **many** entails **some**, etc.

An implicature can be derived by flouting the maxim of Quantity is:

There are three things which are too wonderful for me,
Four which I do not understand. (Proverbs 30:18)

The first line would implicate that there are no more and no less than three things beyond comprehension even though there is always the possibility that there could be more. The second line flouts the maxim of Quantity in respect to the first line and implicates that there are actually more than three, namely four. This may be a common feature of Hebrew poetry as an attention getting device; similar expressions are used in the same chapter and elsewhere in Proverbs.

In the sections below, we will look at conversational implicatures based on the various maxims and how each of these may be flouted to derive other implicatures. It is not always possible to clearly delineate which maxim or maxims are involved in some implicatures. As Sadock (1978 in Davis 1991:368) notes:

> So powerful is each of the maxims that at times they vie for the privilege of explaining the same facts.

Conversational implicatures are also divided on another parameter. *Generalized* implicatures are ones which derive with no special context or scenario (Levinson, 1983:126). **I walked into a house** requires no special context to implicate that it was not my house or I would have said **my house**. *Particularized* ones, however, do require a special context to derive them, as in the bank example above. Some analysts do not consider the generalized ones to be implicatures at all. Carston (1988 in Davis, 1991:34) refers to them as explicatures, but Recanti, (1989 in Davis, 1991:107) divides **what is said** into **sentence meaning** and **contextual ingredients of what is said**, and, as I understand him, includes the generalized implicatures in his 'contextual ingredients of what is said.' Although I find the arguments of Carston and Recanti very appealing, for practical purposes I am leaving the generalized implicatures here.

Another consideration is what Morgan (1978 in Davis, 1991:250f) refers to as a *short-circuited implicature*. This is an implicature which is regularly used with a specialized meaning and has become conventionalized to the extent that a person does not have to make any inferences to figure it out when he hears it, but which still maintains its literal force such that a person who had not heard its conventionalized meaning would be able to infer what the implicature is. An example Morgan gives is: **If you've seen one, you've seen them all**. One could say, **They're all alike, so it's a waste of time to examine them separately**, or something similar, but how many native

speakers of English would not use the former which, although conventionalized, still maintains its literal meaning? A short-circuited implicature may eventually reach the place where only the conventionalized meaning is recognized and as such is an idiom. There is a continuum between being a short-circuited implicature and a fully fossilized idiom. For different speakers, a particular one might be at different points on the continuum. Where is **Let bygones be bygones** on the continuum for you? I think I could figure it out if I weren't familiar with the idiom (my dictionary says it is an idiom) even though I would not use the word **bygone** in any other context, but I had to be taught that **Goodbye** comes from **God be with you**.

Generalized conversational implicatures are fairly easy to identify since they tend to be universal (see comments on scalar implicatures below under Maxim of Quantity). It is, however, often not easy to identify particularized implicatures and Levinson notes:

> ...as anticipated by Grice, there do seem to be additional non-conventional kinds of inference produced by different maxims or principles of language usage. For example, ...principles of politeness that produce systematic inferences of intriguing complexity (Brown and Levinson, 1978). Indeed, there may well be a general principle here: for every kind of mutually assumed constraint on language usage, there will be a corresponding set of potential inferences that come about either from the speaker observing or flouting the constraint. If this is so, there are many kinds of implicature yet to be discovered (1983:131f).

A number of writers recognize the need for a consistent means of testing to determine whether conversational implicatures are genuine or not. Grice has suggested several tests and although some are helpful all of them have been challenged (see especially Sadock, 1978 in Davis, 1991). Three which are helpful, especially if taken together (cf. Sadock, 1978 in Davis, 1991:367,374 and Levinson, 1983:119) are:

1. Calculability—they can be figured out on the basis of the cooperative principle.

2. Cancelability (defeasibility in some of the literature)—they can be voided or nullified (some floutings of implicatures are apparently actually intentional cancellations—see example from Proverbs above).

3. Non-detachability—what they infer is based on meaning rather than on form.

Keeping these tests in mind may be helpful, but working through the questions and following discussions in the sections below should be more productive. It will not lead to identifying all possible implicatures, but should enable you as analyst to get a better feel for implicatures in your own language and how for translation purposes they might relate to implicatures in a target language.

Since the primary domain of implicatures is conversation, it would be productive to take a few texts which are basically conversational in form or which contain sections of conversation and seek to identity as many of the implicatures as possible.

Under each section below, information is given which should help to identify the various implicatures. A few general heuristic devices that may be helpful in identifying conversational implicatures are:

1. Note any incongruencies in responses in conversation. Then, assuming the respondent is not violating the Cooperative principle, seek to identity the cause of the incongruity.

2. When there is obvious omission of information, write out the propositions which provide the information the speaker presumably intended. Each will contain at least one implicature. See the first example under the Maxim of Relevance.

3. Reduce propositions to simpler forms. For example, a question will have one implicature because it is a question and may have another based on other factors. Restating the question as a statement should make the other(s) easier to see. 'Can you tell me the time?' can be restated 'You can tell me the time'. This makes it clear that there is a genuine implicature of asking the person if he is able to tell the time, but makes it more apparent that there is also an implicature of request to tell the time.

4. Look for implicatures that are different because of cultural differences. Bruce Hooley (personal communication) mentioned the problem in translating the Gospel of Mark where Jesus instructed that the girl he had just healed be given something to

eat (Mark 5:43). In English there is an implicature that she was given something to eat or we would have been told otherwise. In the particular culture Hooley referred to, the implicature was that she was not given anything to eat since it was not explicit.

MAXIM OF QUANTITY

Implicatures based on the maxim of quantity can be quite varied. Two areas, however, have been more thoroughly examined: scalar quantity implicatures and clausal quantity implicatures. Both of these are considered to be generalized quantity implicatures; they do not require special context conditions in order to be inferred (Levinson, 1983:104). Here I will treat these two areas plus a category of other quantity implicatures. Levinson suggests that generalized implicatures are probably pretty much universal so that we would not expect a lot of variation from one language to another. Remember that the subpoints of the maxim of quantity are:

> Make your contribution as informative as is required for the current purposes of the exchange.
>
> Do not make your contribution more informative than is required.

SCALAR QUANTITY IMPLICATURES

Scalar quantity implicatures are derived when there are two or more options on a scale and when the use of one of the options implicates that that option is the maximum information on the scale that the speaker can affirm. The option which he chooses also entails any option lower on the scale, if any. On the scale of <all, most, many, some, and few>, the use of *many* implicates that the speaker knows that neither *most* nor *all* is appropriate or that he simply does not know enough to state a higher option. His use of *many* also entails that whatever he said is true both of *some* and *few*.

Research in scalar options of a language should show which options would produce scalar implicatures. Some possible scalar options for English are:

> Numerical
> <n, ... 3, 2, 1>

Quantitative
<all, most, many, some, few>
<none, not all>

Temporal
<always, usually, often, sometimes, seldom>
<never, not often>

Sentential
<necessarily, probably, possibly>
<certain that, probably, possible that>
<impossible that, unlikely that, uncertain that>
<and, or>
<must, should, may>

Degrees of a quality

<excellent. good>	<bad, poor>
<hot, warm>	<cold, cool>
<love, like>	<hate, dislike>
<huge, big>	<tiny, small>

The language may not have some of the scalar options that English has, but may have some that English does not have. Solomon Islands Pijin does not have either the <hot, warm> or <cold, cool> scale. There may also be more or less options on any particular scale. Solomon Islands Pijin has only <*evri*, *plande*, *samfala*, *lelebet*> in its <every, many, some, few> scale. English could have more on some of the scales such as *lukewarm*, *tepid* on the <hot, warm> scale.

Research should ask, in what ways does the language derive implicatures by observing the maxim of scalar quantity?

See the example of Noah above for an example observing a numerical scale. To say that some of the boys went swimming implicates that the speaker either knows that not all or even most of the boys went swimming or that he did not know enough to make a stronger statement since to be cooperative he would have used a stronger statement had he known it to be so. To say 'It is unlikely that I will live to be 100' implicates that I do not expect to live that long, but it does not mean that it is impossible. It also entails that it is uncertain

whether I will live to be 100. (I don't know of any of my ancestors who lived to be more than 84.)

As a research technique, you can ask if there are ways in which scalar implicatures in the language are different from the English examples. In Solomon Islands Pijin the scalar implicature is much more frequently reinforced than in English. In Pijin it would be natural to say *Noa hemi garem trifala boe pikinini nomoa* 'Noah he had three boy child only'. To delete the *nomoa* would be possible but often less natural. (It may be that Hebrew differs from English in that in many of the Old Testament genealogies there is only a mention of sons. In English this would tend to implicate that the person did not have daughters, but in Hebrew there apparently is no such implication.)

You might also ask, in what ways can the scalar implicatures be flouted in the language? For what special purposes? See the preceding example from Proverbs in which the quantity of three was first expressed and then purposefully upped to four as a device to gain attention. Scalar implicatures may be among those most commonly flouted and that most commonly for strengthening, e.g. 'It's warm in here, in fact it's hot'. In these types of cases, it is evident that the speaker is observing the Cooperative Principle.

Scalar implicatures may be flouted to implicate an obvious understatement (meiosis) as in 'Don't you think it's a little warm in here?' where the implication is that it is hot, possibly unbearably hot. This could further implicate a request to open the door, turn on the air conditioner, or turn the heat off. Scalar implicatures may also be flouted as an obvious overstatement (hyperbole) as in 'Everyone's doing it' when as a matter of fact the speaker is seeking to justify what he is doing or wanting to do making the real implicature 'It's ok for me'. (Presumably Grice (1968 in Davis, 1991:312) would consider these as violations of the maxim of Quality. It is possible, of course, for more than one maxim to be flouted at the same time; clearly, however, quantity is involved in these.)

Implicatures can be and are frequently flouted in adversarial court systems as in the U.S. Levinson (1983:121) illustrates violation of a quantity implicature with the following cross-examination of a defendant (W) by legal counsel (C):

C: On many occasions? W: Not many
C: Some? W. Yes, a few.

C does not assume that W has observed the maxim of Quantity in his first response. Rather than implicating *some*, W could mean *none*. C proceeds with a second, weaker question and W, unwilling to admit even that much, weakens the desired implicature of *some* to *few*.

CLAUSAL QUANTITY IMPLICATURES

Levinson (1983:136) notes:

> The underlying intuition [of clausal implicatures] is this: If I use some linguistic expression that fails to commit me to some embedded proposition, in preference to another available stronger expression that would so commit me, then I may be taken to implicate that I am not in the... position to make the stronger statement.

These implicatures are clear from constructions using pairs such as *know* and *believe* in:

> I know the Cowboys will win the Super Bowl.
> I believe the Cowboys will win the Super Bowl.

The second sentence implicates that the Cowboys might win and also that they might possibly not win.

Seek pairs of words (or other forms) that the language uses which enable one to derive clausal quantity implicatures. Some of these will be the same as in the scalar options. Some pairs with the stronger form listed first are (see Levinson, 1983:137):

and	or
since, ...then	if, ...then
know	believe
realize	think
reveal	say
necessarily	possibly

The use of *or* or *if* yields four implicatures since each involves two clauses. In 'Dad or Mom will pick me up' the four implicatures are: 'Dad may pick me up, Dad may not pick me up, Mom may pick me up', and 'Mom may not pick me up'.

Check for flouting: In what ways can clausal implicatures be flouted in the language? For what special purposes? As with scalar implicatures, they can be flouted by using a stronger form. I could strengthen the statement about the Dallas Cowboys with: 'I believe the Cowboys will win the Super Bowl; in fact, I know they will.' In this example, as in the example from Proverbs, the original implicature has been cancelled according to the procedure for testing for conversational implicatures (see introductory section on conversational implicatures). I would argue that this is nonetheless a type of flouting the original implicature.

Clausal quantity implicatures and perhaps others can be flouted for the purpose of politeness or avoiding confrontation, that is, a person may deliberately use a weaker form when he could affirm the stronger. I will often use *I believe* or *I think* when I am certain enough to use *I know*. This is also a type of understatement.

OTHER QUANTITY IMPLICATURES

If one says 'The flag is white', he implicates that with respect to color the flag is entirely white. If he says, however, 'I bought a brick house', he only implicates that the basic external material of the house is brick. He does not implicate that the house is made entirely of bricks because he expects the hearer to share a common understanding of what a house is and how houses are categorized. He has observed the maxim of quantity by providing the information that would reasonably be expected. One should therefore seek to find quantity implicatures and ways in which quantity implicatures can be flouted, and for what special purposes.

Tautologies are a common type of violating the maxim of quantity according to Levinson (1983:110f). He lists the following:

> War is war.
> Either John will come or he won't.
> If he does it, he does it.

A common one in English is 'Boys will be boys'. Queen Esther's famous statement (in Aramaic) in Esther 4:16 when she decided to approach the King in violation of the law is a good example: 'And if I perish, I perish.'

Levinson says these are blatant violations of the maxim of quantity which requires that the contribution be informative. At the truth-conditional level they are not informative, but each implicates something based on the context of utterance so that the content of the implicature is probably determined by the maxim of relevance. Levinson says that they all have a topic-closing quality. 'If he does it, he does it' probably implicates that it is none of our business and clearly intends to end the matter. 'Boys will be boys' implicates that there are a lot of things boys will do that adults don't do or approve of, but that there really isn't anything we can (or should) do about it. And Queen Esther was likely being fatalistic or stoic.

Along a different line, if Johnny comes home and his mother asks where he has been and he replies 'At the library', he implicates that apart from getting to and from the library, the library is at least the primary place he has been. If, however, Johnny was only at the library for a few minutes and had spent most of this time at the pool hall, he was deliberately violating the maxim of quantity even though what he had told was true. He has not observed the cooperative principle and provided all the information that might reasonably be required.

Maxim of Quality

The maxim of quality requires that the speaker make his contribution one that is true. The two subpoints of this maxim are:

> Do not say what you believe to be false.
> Do not say that for which you lack adequate evidence.

If, for example, I say 'My wife doesn't drink coffee with caffeine in it', I implicate that I believe that my wife doesn't drink coffee with caffeine it and I have adequate evidence that she doesn't.

Levinson (1983:105f) notes three areas of quality implicatures:

1) When someone asserts something, he implicates that he believes it.
2) When someone asks a question, he implicates that he sincerely desires an answer [which he does not already know].
3) When someone makes a promise [politicians not excepted], he

implicates that he intends to keep it.

4) When someone makes a request he sincerely needs or desires it.

A gullible person is one who believes that everyone regularly observes the maxim of quality—and the gullible person is probably faithful to observe it himself. Instances of observing the maxim of quality are many, but floutings of the maxim are without doubt more interesting.

Research the ways that the language flouts the maxim of quality and determine the special purposes of the floutings. In English these floutings are many. The four areas of quality implicatures listed above yield different types.

Ironies are floutings of the first, namely that when one asserts something, he implicates that he believes it. Levinson gives the following example (1983:109):

A: What if the USSR blockades the Gulf and all the oil?
B: Oh, come now, Britain rules the seas!

As Levinson notes, any reasonably informed participant will know the second part of the exchange is false, but that the speaker is not being intentionally uncooperative so he really implicates the opposite. Intonation and other phonological features may help to signal ironies.

Classroom questions and catechizing are good examples of violating what questions normally implicate. In Solomon Islands Pijin, if the questioned person does not know the answer, he will normally reply with *Mi no save tu* 'I don't know either', indicating that he assumes the questioner is observing the maxim and does not know the answer. In the classroom, the teacher regularly asks questions to which he/she already knows the answer, but for which an answer is still expected. In catechizing, religious dogma is taught by questions to which the respondent is taught fixed answers. Are either of these two types of questions used in the language?

Rhetorical questions are classical violations of the maxim of quality. Not only does the one who asks the question know the answer, he does not expect the hearer to answer. Not all languages have them and they are used for different purposes in different languages. In

the Solomon Islands many languages use rhetorical questions only for scolding.

Questions are also frequently used to implicate requests. This flouts the normal implicature, but politely (normally) makes the request.

Implicatures based on promises can be flouted by the speaker having no intention of keeping the promise. In this case he has also flouted the Cooperative Principle. The same would be true of threats which the speaker has no intention of carrying out. At another level, however, the speaker may be being cooperative in that his threats may implicate only strong requests and his promises, which he does not intend to carry out, may implicate politeness.

Implicatures based on requests can also be flouted by insincerity but, again, could be either cooperative or uncooperative depending on whether the speaker was implicating politeness or was trying to take advantage of the hearer.

Maxim of Relevance

This maxim is the simplest: Make your contributions relevant. It does, however, result in a large range of standard implicatures.

The maxim of relevance is of utmost importance in considering interactions where a contribution may superficially seem to be irrelevant. Levinson (1983:107) illustrates this well with the following interaction:

A. Can you tell me the time?
B. Well, the milkman has come.

First, B's response implicates that he believes and has evidence that the milkman has come (a quality implicature). We also assume that he is being relevant. His response implicates that he is unsure of the time but is trying to provide a partial answer to the question. He assumes the one who asked the question knows about what time the milkman comes so the present time must be later than that. [Note the implicature in the question; it flouts the normal implicature of the question and implicates a request for the hearer to tell the time.]

What is important is that the participants must assume that what the other participant(s) contribute is relevant unless there is some clear indication that they are being uncooperative.

First find examples/types of relevance implicatures in the language, then determine ways in which speakers can flout the maxim of relevance.

Contributions, which appear to be irrelevant but where there is no evidence that the participant is being uncooperative, will normally implicate something that is relevant. (See the example above with the response about the milkman.) If a hearer fails to see the relevance of a contribution, he may respond with something like 'I don't get you' or 'What's that got to do with anything?' The ability of participants to determine the relevance varies significantly; some people expect more literal cooperation than others.

Levinson (1983:107) suggests that imperatives are a type of relevance implicatures because, if possible, they will be interpreted as relevant to the present interaction and, as such, requests to do something at that time. 'Pass the salt' implicates 'Pass the salt now'.

Levinson and Grice note that examples are harder to find than for other maxims, but a fairly common one is the introduction of something irrelevant to change the course of a conversation. Levinson (1983:111) gives the following example:

> Johnny: Hey, Sally, let's play marbles
> Mother: How is your homework getting along, Johnny?

The implication here is a reminder to Johnny that if his homework isn't finished, he is not free to play.

MAXIM OF MANNER

Submaxim: avoid obscurity of expression

This assumes that a speaker will not intentionally be obscure in what he is stating, but will state clearly what he intends to communicate.

If you can identify ways the language derives implicatures by observing the submaxim of avoiding obscurity, seek also the ways the submaxim of avoiding obscurity can be flouted in the language.

Examples of observing this submaxim are common but not particularly interesting. Direct commands or requests are perhaps the most obvious of nonobscure implicatures as in 'Shut the door'. These were also given as examples for the maxim of quality, but there it involved the genuine expectation that the person asked to do something would perform it and that the person requesting it genuinely wanted it done.

Floutings of this submaxim are much more interesting than observances of it. Obscurity may be employed to keep some hearers from understanding as, for example, when a parent spells a word to keep the children from understanding or when a person introduces a foreign word which his target audience will understand, but which others will not understand. Jesus deliberately spoke in parables so that many of his hearers would not understand. Obscurity of expression can also be used as a means of getting the other participants to think about an issue rather than being spoon-fed every detail. In other cases the use of metaphors could be examples of deliberate obscurity if the hearer is not expected to understand the significance of the metaphor. Note that if the hearer is intended to get the wrong meaning through obscurity, it is a flouting of the maxim of quality as well as a flouting of the maxim of clarity.

Apocalyptic writings will often flout the maxim of clarity in order to have veiled references to people and places in the current political setting. Obscurity can also be a means of being polite since a direct statement in many cultures is understood to be impolite or at least not adequately polite. One may say, 'I am available whenever you would like to talk to me' when the real intent is to say, 'I would like to talk with you as soon as you can make yourself available'. There may be some flouting of the maxim of quality as well, but it seems that it is primarily being less than clear.

Conversation also includes a lot of unintended obscurity. The speaker either does not realize that he is being obscure or does not know how to state what he wants to communicate clearly, and that may be the primary implication of what he says. Unintended

obscurity is not a flouting of the maxim of clarity since the speaker is observing the Cooperative Principle.

Submaxim: avoid ambiguity

This submaxim assumes that the participant in a conversation will normally state what he intends to communicate in a way that does not offer a choice of two or more different meanings.

The participant in a conversation may actually use a statement which could be ambiguous out of context but which is perfectly clear in the context in which it is made. The statement would then implicate what the speaker intended. For example, in English, the word *church* may be used in a variety of ways to indicate a building, a local congregation, and the total group of adherents to a particular denomination or religion. Although all three of these meanings are related, the first is less closely related to the second two than the second two are to each other. 'The church on Main Street' would normally implicate a building whereas 'The church adopted a new Confession of Faith' would implicate the adherents of a denomination or some other large group.

The ambiguity may involve more than a single word. The statement, 'I haven't been to class yet' could mean that I haven't been there yet today or it could mean that I have not yet attended any class of a particular course of study. In response to the question 'How are you enjoying Linguistics 101?' the above statement would implicate that the speaker had not been to any L101 class yet and not specifically that he hadn't been there yet today, even though it would be true by virtue of entailment. It also implicates other things since a more complete answer might be 'I don't know whether I will enjoy it since I haven't been to class yet'.

If you can identify ways in which the language derives implicatures by observing the submaxim of avoiding ambiguity, seek also to find ways that the submaxim of avoiding ambiguity can be flouted. Some puns and jokes make deliberate use of ambiguity. The following example from the January 1993 *Readers Digest* would indicate that although the salesclerk was observing the submaxim, the customer was miscued and came up with an inappropriate response:

> ...I overheard a young couple interested in purchasing a cordless telephone. The salesclerk showed them a model that had a range of over 100 feet. 'That won't do,' the young woman said. 'I have relatives in Philadelphia.' (*Readers Digest*, January 1993:72).

Submaxim: Be brief

This submaxim assumes that the participant will not normally be wordy (prolix). Observing this maxim may have considerable range since what is brief for one person may be inadequate (unclear or ambiguous) for the next person and what may be wordy for one person may be just right for another, but there are some bounds.

A question to ask is, in what ways is the submaxim of brevity observed to derive implicatures in the language? The expression 'Please open the door' implicates that the speaker expects the hearer to know the details necessary for opening the door and that he will perform those details. The speaker is not observing the maxim if he says, under most circumstances, 'Walk up to the door, reach out with your right hand, take hold of the door handle, turn the handle clockwise as far as it will go, and then pull the door towards you' (modified from Levinson, 1983:108).

Certain commonly accepted expressions can be used to attain brevity and still communicate far more than the sum of the parts. The references to 'killing the fatted calf' in the story of the Prodigal Son in Luke 15 in the Bible is a very terse way to implicate all that goes into the preparation of the feast and that the one for whom the calf is killed is the subject of honor. This is what Morgan (1978 in Davis, 1991:250) would call a short-circuited implicature if we assume the phrase has become conventionalized, but that at the same time the implicature can be worked out from the literal statement.

Wordiness is a type of redundancy but not all redundancy is wordiness. Languages employ a lot of natural and often obligatory redundancy although the types of redundancy will vary from language to language. English, for example, requires that a transitive verb have a grammatically explicit patient or undergoer as in 'Grandma made it' whereas the Solomon Islands Pijin equivalent would be '*Grani hemi wakem* 'grandma 3sg made' with no overt patient if already clear from the context. Pijin, on the other hand, normally

repeats the pronominal subject, hence the *hemi*. ['Grandma she made it' is, of course, acceptable in some varieties of English.]

The next question would be, in what ways can the submaxim of brevity be flouted in the language? For what special purposes? The submaxim may be flouted when necessary to give detailed instructions as in teaching someone a new skill. It would be very unusual to give detailed instructions in how to open a door as given above, but details for opening the door of a safe with a combination lock could well be that detailed. The implicature is that the details are necessary to observe in order to obtain the desired results.

The submaxim may also be flouted to implicate a sharp contrast in meaning of a wordy statement as compared with a brief straightforward statement. Levinson (1983:112) gives this example:

> Miss Singer produced a series of sounds corresponding closely to the score of an aria from *Rigoletto*.
>
> Miss Singer sang an aria from *Rigoletto*.

The wordy statement implicates that there was a sharp difference between what Miss Singer did and what singing is.

Submaxim: Be orderly

Levinson believes this may be the most important submaxim. The commonly cited application of this maxim is that events will be recounted in the order in which they happened or are expected to happen. Thus 'Get your pajamas on and go to bed' implicates that the pajamas are to be put on before getting in bed. It is because we expect orderliness in a statement that it implicates the sequence. The implication can be made explicit by the addition of *then*, but in English is often not explicit. There are cases where *and* joins clauses with events which occur simultaneously or where both simply happen to be true. The pragmatic determination of these from context eliminates the need for describing the language as having a word *and* which states that two facts are true, another *and* which states that two events are simultaneous and yet another *and* which adds the notion of sequentiality. The difference is not because of different *ands* but different implicatures because of differences in the context.

You may be made aware of orderliness patterns by being misunderstood if you don't follow them. In what ways does the language derive implicatures by preserving the submaxim of orderliness? Probably the most common ones will be that of the sequencing of events. Does the language require an overt indicator of sequencing other than the order in which they occur?

Orderliness may also be maintained through overt signals which indicate the correct order of events which are not in strict chronological order. Such signals could be: **meanwhile, at the same time, while, formerly, before**, etc. For example, 'Before Clinton became President, Hillary sought to maintain a low profile'.

In what ways can the submaxim of orderliness be flouted in the language? For what special purposes, if any? In many cases a flouting of this submaxim would result in nonsense or unacceptable sentences. It is hard to imagine intentional floutings of the submaxim which are not intended to deceive or confuse the hearer and to do such would be an infringement of the Cooperative Principle.

Conventional Implicatures

Conventional implicatures contrast with conversational implicatures in that they are 'non-truth-conditional inferences that are *not* derived from superordinate pragmatic principles like the maxim, but are simply attached by convention to particular lexical items or expressions' (Levinson, 1983:127).

Grice, who introduced the notion, did not himself develop it to any extent. He gives only two examples: *but* and *therefore*. The word *but* has the same truth-conditional content as *and* with an additional implicature of contrast. And *therefore* does not contribute anything to the true conditions of the expression in which it occurs. Levinson (1983:127) cites other authors as suggesting *even* and *yet*.

Levinson (1983:128) acknowledges that:

> In a sense conventional implicature is not a very interesting concept—it is rather an admission of the failure of truth-conditional semantics to capture all the conventional content or meaning of natural language words and expressions.

Levinson proceeds to note two types of deictic expressions which he says have conventional implicatures as a central meaning component. These are discourse deictic items (e.g. *however, well*) and socially deictic items (e.g. *sir, your honor*, etc., when used in address—see example of French *tu/vous* where the notion of conventional implicature was first introduced). The conventional implicatures can be expected to differ from conversational implicatures with regard to each of the tests for conversational implicatures discussed above. They cannot be calculated by using pragmatic principles (e.g. one does not use context to determine what *tu* or *vous* implicate), they will be detachable because they depend on the specific linguistic items used (e.g. *tu* versus *vous* where the implicature changes with the word but the truth value of the proposition remains the same), and they cannot be cancelled without being contradictory or anomalous.

Conventional implicatures further contrast with generalized conversational implicatures in that they to not tend to be universal; they may vary considerably from language to language.

What discourse deictic items does the language have which derive conventional implicatures? Those that in some way relate an utterance to surrounding text. Examples in English are *however, moreover, besides, whereas, on the other hand, anyway, well, still, furthermore, nevertheless, oh, so, but* (initially in objecting), *and* (initially in eliciting more information), etc. One of the usages of *oh* as an initial particle is used by one speaker to signal that he has just heard what the other person has communicated. The *oh* in itself has no propositional content. Differences in intonation or other features of how the *oh* is spoken will also enter into the total intent of the speaker. Many more utterance initial response words with conventional implicatures can probably be found by studying conversations in texts.

What socially deictic items does the language have which derive conventional implicatures? This includes all address terms which may signal a social relationship such as *sir, madam, bro, brother, sister, mate, your honor, your majesty, sonny, sweetheart, sugar, hey*, etc. It would also include the honorifics of languages in various parts of the world and pronouns of the *tu, vous* type in French.

Can conventional implicatures be flouted in the language for special purposes? It does not seem to me that it would be possible to flout

discourse deictic items without creating nonsense, but socially deictic items are frequently flouted as put-downs. 'Your majesty', for example, is used in addressing someone who assumes authority that does not belong to him. There may also be other ways in which the socially deictic items can be flouted.

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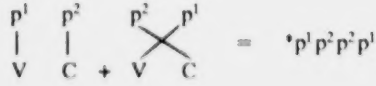
Mirror-Image Reduplication in Amele

John R. Roberts

1. Introduction

In the range of reduplication phenomena found in the Amele[1] language discussed in Roberts (1991) I described several types of such that could be analyzed as so-called 'mirror-image' reduplication, i.e. where a linear string is reduplicated but the segments in the formant are in reverse order to the segments in the base string. This was an interesting observation since several linguists, such as Moravcsik (1978) and Marantz (1982), have claimed that this phenomenon does not or should not occur in natural languages. In the case of Marantz's theory of nonlinear phonology the primary principle that linking lines must never cross is, of course, broken by examples of mirror-image reduplication. In (1), for example, the association lines from the CV skeleton to the segments in the reduplicated formant would be crossed in the case of mirror-image reduplication.

(1) 

In this squib I review the data presented before in Roberts (1991) and also present fresh data that confirm the existence of mirror-image reduplication in Amele.

2. Data Review

In Roberts (1991) I described types of reduplication in Amele on the locative pronouns and some of the postpositions that could be analyzed as mirror-image. Examples of these are given in Table 1.

Table 1. CV → VC Reduplication on Locative Pronouns and Postpositions

ene	'here'	ene-ʔ-en	=	ene-ʔ-in	'it is here'
ono	'there'	ono-ʔ-on	=	ono-ʔ-in	'it is there'
ana	'where?'	ana-ʔ-an	=	ana-ʔ-in	'it is where?'
ijana	'my'	ijana-ʔ-in			'that is mine'
ijanu	'for me'	ijanu-ʔ-un			'that is for me'

The primary function of this mirror-image reduplication is to express emphatic focus, as in (2) for example.

(2) a. Ho eu ija-na.
pig that 1S-POSS
'That pig is mine.'

b. Ho eu ija-na-ʔin.
pig that 1S-POSS-EMPH
'That pig is (definitely) mine.'

While the semantic function of the mirror-image reduplication is basically one of emphasis it also has the derivational function of forming a predicative adjective from a pronominal. Whereas the possessive pronoun form can function either as an attributive or predicative adjective as in (3), the mirror-image reduplicated form can only function as a predicative adjective as in (4).

(3) a. ija-na ho
1S-POSS pig
'my pig'

b. ho eu ija-na
pig that 1S-POSS
'that pig is mine'

(4) a. *ija-na-ʔin ho
1S-POSS-EMPH pig

b. ho ija-na-ʔin
pig 1S-POSS-EMPH
'that pig is (definitely) mine'

Similarly, the demonstrative pronoun can function either as a demonstrative or predicative adjective as in (5) but the mirror-image reduplicated form can only function as a predicative adjective and not as a demonstrative, as in (6).

(5) a. jo ono bil-i-a
house there sit-3S-TODP
'the house is there'

b. jo ono
house there
'the house is there'

(6) a. *jo ono-2on bil-i-a
house there-EMPH sit-3S-TODP

b. jo ono-2on
house there-EMPH
'the house is (over) there'

The analysis that the above examples of reduplication are mirror-image is controversial, however. Firstly, the CV → VC process requires glottal stop epenthesis and secondly, the CV → VC process allows a vowel change in all except the *nu* → *nu un* case. The question then arises as to whether this is really mirror-image reduplication or whether we just have a suffix, such as *-Vn*, attaching to these forms.

In Roberts (1991) I presented evidence that the glottal stop could be epenthetic by the fact that there is a correspondence between forms in other languages of the Gum language family and Amele which show that metathesis has taken place in the Amele form and where metathesis has taken place there is often a glottal stop inserted. Examples of these metathetic correspondents are reproduced in Table 2.

Table 2. Metathetic Correspondents between Amele and Other Gum Languages

Amele Form	Form in Other Gum Languages	
bui2	biw	'ripe'
esi2	egis	'sand'
gola2	joga	'blood'
igo2	go2in	'peak, summit'
le2is	elis	'two'

Additionally, it is also the case that a number of words in Amele have an optional epenthetic glottal stop. Some examples are given in Table 3.

Table 3. Forms with Optional Epenthetic Glottal Stop

leis	=	le?is	'two'
maun	=	ma?un	'cold'
meeg	=	me?eg	'dry'

All of this would suggest that there is some phonological basis for a rule of glottal stop insertion in the environment of CV + VC to maintain the integrity of the vowel in the mirror-image formant.

The vowel change in the mirror-image reduplicated forms is less of a problem, since vowel change occurs in other environments of reduplication such as irregular iterative verb forms, as in Table 4 for example.

Table 4. Vowel Change in Amele Reduplication Processes

budue?	'to thud'	budu-badae?	'to thud sporadically'
gasue?	'to search'	gasu-gisie?	'to search here and there'
?a?agane?	'to talk in sleep'	?a?agan-?u?ugune?	'to talk sporadically in sleep'
lahido?	'to shake something'	lahi-luhudo?	'to shake something all over'
filihi?do?	'to unravel something'	filihi?-foloho?do?	'to unravel something all over'
dido?	'to pull'	di-dado?	'to pull carelessly'
?ogoge?	'to twist'	?ogog-?igige?	'to twist all over the place'
me?ie?	'to look'	me?i-mue?	'to look from side to side'

Although this evidence is persuasive it does not substantiate that the forms in Table 1 are clearly examples of mirror-image reduplication.

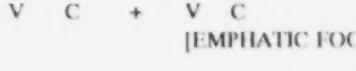
3. Clear Mirror-Image Form

Since publishing Roberts (1991) a clearer form of mirror-image reduplication in Amele has come to my attention. This is where the interrogative pronoun in 'who(sg)' reduplicates to *inni* 'who(pl) is it'. Again the function of this reduplication process is to express emphatic focus. An example of the form *inni* occurring in extended text material is given in (7).

(7) Wele inni age Anut jejeg doin,
previously who-is-it they God his-voice they-heard

odimeig fileeeig uqa jejeg wooldoin?
they-did-and as-they-disobeyed his voice they-by-passed-it
'Who is it that previously heard God's voice and then disregarded it and disobeyed it?'

The *inni* form would therefore be an example where association lines cross over in the reduplication process, as illustrated in (8). However, in this case the form produced is perfectly grammatical and not ungrammatical as predicted by Marantz's theory of nonlinear phonology.

(8)


4. Conclusion

The job of theoretical linguists is to devise theories that better account for the data. The job of descriptive linguists is to highlight data that are not accounted for by the theorists. Thus linguistic theory progresses.

NOTE

[1] Amele is a Papuan language spoken by approximately 6001 people who live mainly in Madang Province just south of Madang itself. A fuller description of the grammar of Amele is given in Roberts (1987). The

abbreviations used in the data examples are POSS(essive), EMPH(atic focus), TOD(ay's)P(ast tense), 1(st person), 3(rd person), s(ingular number). All the data examples are in phonemic transcription. The symbol = means 'varies with'.

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Response to E. Lou Hohulin's review of my

Relevance relations in discourse: A study with special reference to Sissala.

Cambridge University Press. 1990. 300 pp. Reduced to $38. (U.S.)

Regina Blass

I very much liked Lou Hohulin's introduction to her review of my book, *Relevance Relations in Discourse* (RRID). It is true that there were a number of issues that bugged me before and while I was working on it. Concerning the particles, I had once experienced a linguist-translator and consultant with MA in linguistics sticking in particles in her final typed NT, at the wish of her translation helper, without knowing why. At the time, it became clear to me that there must be other criteria that guide the appearance and function of those phenomena than the ones we had hitherto applied. Looking for those answers rocked the very basis of what I thought discourse analysis was about. This is why I dealt with more general issues of discourse, especially with the issue of relevance relations in discourse in the first chapters.

Except for minor details I feel Lou Hohulin has given a fair summary of RRID with some interesting conclusions which, however, I would have liked a bit more directly related to my discourse and particle analysis. I will come back to that later. First I would like to comment on some of her objections and questions.

'Hearsay' particles and interpretive use

I have never claimed that a language that has hearsay particles, indicating reported speech, must necessarily mark repetitions, questions, and irony.

It is of course possible that a language marks only reported speech with one marker optionally, as it seems to be the case in Philippine languages, according to Hohulin. This does not disprove that what has traditionally been called a 'hearsay marker' is not indicating interpretive use. It is also quite possible that a language has a number of different ways to indicate interpretive use, or there may be

languages that do not mark it at all. In English we find interpretive use marked in various ways, such as with *indeed* which, no doubt, indicates that a proposition will be echoed and with the adverb *supposedly*, while in French and German the conditional is exploited for these purposes.

It was, of course, exciting to see that a language like Sissala marks with one marker practically every aspect that Sperber and Wilson had before identified as interpretive use. In fact I considered it one of the most important findings of my research.

Even though Hohulin was not successful in finding the same markers for reported speech as for reported thought, questions and answers, or repetitions for the sake of expressing agreement or disagreement, this does not mean that it has not been found anywhere else in the world. In fact, since RRID has come out, Itani Kaufmann (a native Japanese) has been working in London on the particle *tte* in Japanese, which is supposedly not only employed in reported speech but also in other uses of interpretive use. In Turkish, where Slobin and Aksu (1982) themselves mention that they have found the hearsay marker *mis* used in irony, more than the strict hearsay uses have been found.

I have asked SIL members working in languages with hearsay markers whether they had found the same marker used for other than strictly hearsay uses. Some immediately said 'No' and others said 'Yes', but they could not give me the details because they had given up paying attention to these phenomena that they could not see as having any relation to this hearsay phenomenon and, being away from the language situation when I asked them, they found it difficult to recall anything specific.

So, knowing Lou Hohulin's review and my answer will reach a number of our members in a village situation, please do let us know if your hearsay phenomena also appears in repetitions, in questions, in answers to yes-no questions, in agreements and disapprovals, in constructions of propositional attitude (such as 'I know that', 'I wish that') and, of course, you can also contact us if your language does not have these uses.

Coming back to Lee Ballard's example (1974) with the particle *kono* in the translation of Matt. 27:42, I am surprised that Hohulin cannot

see that even if a marker does indicate 'hearsay' it also can mark irony, because irony IS hearsay. As I already mentioned, Slobin and Aksu (1982) with their Turkish *mis* example noticed that hearsay can have the effect of irony. They noticed that even independently of any claims about interpretive use.

What makes me think that Hohulin is probably not right in claiming that the whole of Matt. 27:42 is just meant to be seen as the reported mocking is the fact that the verse has two markers of *kono* in the middle of the sentence indicating obviously a scope, and that scope looks suspiciously like the scope of the irony.

> He saved others *kono*, but he cannot save himself. He is *kono* the king of Israel, so let him come down from the cross.

'He cannot save himself', does not have a marker *kono*, neither does 'so let him come down from the cross'. Those parts of Matt. 27:42 are not ironical. I am surprised that Hohulin has not commented on that.

It is definitely true that we are at the beginning of a particular research in this area, and we have not found a lot of language phenomena similar to Sissala and Japanese yet. It could be that these languages are unique and rather idiosyncratic to mark a number of phenomena identifiable as interpretive use. However, I am not sure that we have done enough research to make final statements. At any rate, whether we find markers or we do not, does not disprove Sperber and Wilson's analysis. I do not see my research as proving their theory. My aim was more to find out which cognitive processes and states are represented by form in natural language, thereby finding some explanations for the many open questions as to what certain forms really stand for in language. Interpretive use seems to be one of those marked in language.

When I was working on the fourth chapter, 'Constraints on relevance and particle typology', I thought I had come up with some very idiosyncratic solutions when I claimed that the relation between the modal and adverbial use of 'also' in Sissala and German and the reason for their common form was that they could not be used in backwards contradiction. Having since looked at other languages and spoken to SIL member Stephen Levinsohn, who has looked at even more than I, it has become clear that my research showed

phenomena which seems to be found in many languages. So the surprises might yet come for the interpretive use phenomena as well, especially if our members could finally be motivated to look at what is still there unexplained, and if they do not just leave it at that.

Research methodology

I am not quite sure why Hohulin feels that I should have given a lot of attention to Sperber and Wilson versus my research methods but I can definitely comment on them now. Sperber and Wilson's main aim was to present a new theory they had developed. They chose the most clear and easy way to illustrate their arguments—they made up language examples. They did this mainly because natural language examples often come with various complications, and they did not want to sidetrack their hearers. Many of their examples are of the kind that someone says, assumes, or concludes something

I, on the other hand, used recorded natural data in which someone says, assumes, or concludes something. Besides culture I do not see any great difference between Sperber and Wilson's and my introspection. We both made use of other people's speech—that is, we used language interpretively. Sperber and Wilson echo imagined individuals while I echo real ones.

Of course there is a great difference between Sperber and Wilson's and my goal in research. Though RRID is about pragmatic theory, it is not a book on cognitive science nor about communication theory as such, as Sperber and Wilson's *Relevance* is. RRID is, after all, about linguistic phenomena and its interpretation in context. I claim that there are linguistic phenomena which only make sense if they are explained in relation to context (assumptions in memory). This is why I have taken the trouble to introduce the theory.

Hohulin asks how I could be sure about my analysis since I am not a native speaker. Surely the linguistic phenomena helped. If I find a particle that prefaces an explicit conclusion like *so* in English or *sie* in Sissala, then it is not very difficult to see what role the proposition plays in the discourse. If I find a particle that occurs consistently after a conclusion and the semantic content of the proposition provides the kind of information that would confirm a preceding

statement, like *after all* in English or *ma* in Sissala, or *auch* in German, then I can come to the conclusion that this marker constrains the proposition it prefaces to be a premise strengthening the preceding proposition. In a way I worked very much like many SIL workers, collecting lots of texts, making lots and lots of discourse charts, looking where the particles occurred, etc. However, whenever I realized that a particle played a role as an interpropositional connective, then I did not just give the propositional relations a label, such as 'head-satellite' or 'grounds-conclusions'. I asked myself what assumption from the context had played or might have played a role, what determines premise and what determines conclusion, and I spelled them out because the explicit propositions do not always provide all the premises on which the conclusion rests.

In fact, in some ways other analysts also take implicit intuitive assumptions into account in their analyses, only they do not spell them out. For instance, in a sentence like

> I am officer Kripke
> and you are under arrest

the second clause may be understood as the conclusion of the first, which in turn could be seen as the premise and justification for the conclusion. However, this conclusion can only be derived by someone who knows something about the authority of an officer, otherwise there is no justification. Basically the analysis as 'justification—conclusion' rests on what I would spell out as premises from the context (i.e. from assumptions in memory).

If I have produced what Hohulin calls 'introspective' material, then I have only spelled out what others consider, too, but do not deal with in theoretical terms. In fact, if someone claims that there is a relation of 'justification—conclusion' in the above example without making reference to further assumptions from the context, then the person has not applied any theory at all. He or she has applied intuition. My approach is to apply intuition and offer a theoretical basis for it.

The problem of many 'relations between proposition analyses' is that the relations often do not exist strictly speaking between certain propositions, they exist between the text and the context. Producing

the contextual assumptions is just as important for the determination of the relation as the overt linguistic phenomena.

Often I also asked myself in my analysis what the communicative goal of the speaker was at that point of the discourse. What did this particle contribute to the relevance of the proposition it prefaces? How did the speaker want to improve the overall representation of the hearer's world? Did he want his hearer to gain new information by drawing conclusions from the new proposition introduced and some assumption from context, or did he want to strengthen an existing assumption, or did he want to weaken or eliminate an existing assumption? Recognizing these contextual effects is understanding the intended message of the speaker or, broadly speaking 'the discourse meaning'. The meaning of the discourse never rests on the propositions expressed alone; it rests also on the context and the contextual effects. Discourse coherence is established by the derivation of contextual effects, not only by cohesive marking and semantic similarity. Many of the markers I dealt with in my dissertation make a direct contribution to discourse coherence, not because they show cohesion, but because they indicate which contextual effect should be achieved. If they show cohesion then this is only an outcome of their primary function of contextual effect indicator.

The difference from work of other discourse workers, those of SIL included, was that next to a structural and functional description I attempted to EXPLAIN in psychological terms the relation between the structure and the function by simply tracing the hearer's path in utterance interpretation. I arrived at the latter by considering my data in the light of hypotheses from Relevance Theory which have, after all, a lot of empirical confirmations, if one reads the cognitive science and artificial intelligence literature on memory organization, etc. In addition, they confirm what people intuitively perceive as 'common sense'.

Regarding Hohulin's quote from Labov, I cannot agree with a lot of what he says. If he wants practical procedures for gathering and processing and reporting data to be approved, then who has the authority to do so and on what basis? Moreover, while I agree that it might sometimes help when many investigators end up with the same analysis, this is by no means a proof that the analysis is right. The

question is on which hypotheses were those analyses based and were they confirmed or disproved. Science always starts with hypotheses. These then have to be confirmed or disproved. Until anything gets seriously disproved the hypotheses are often treated as if they were facts. In many areas of science nothing but hypotheses are the basis on which we understand the world today. So there is nothing disturbing about working with hypotheses. Moreover, some of the hypotheses of Relevance Theory have an empirical basis, so if linguistic data falls in with these explanatory hypotheses, then we are on the way to being able to say what is right or wrong, and we have some empirical testing factors of the kind Labov is looking for, I guess.

I am surprised that Hohulin is concerned in this respect about MY analysis, since I feel others do not even attempt to explain their linguistic analyses; they are quite happy with one possible description, thus being extremely far away from the standards Labov is trying to set.

Moreover, I am surprised that Hohulin as an SIL translation consultant is questioning me on the validity of making use of other people's assumptions, assumptions of people from other cultures, and that I operate with introspection. Is that not what SIL does all the time with its idiomatic translations? Idiomatic translators assume what the background assumptions of the original speakers are and often make them explicit together with conclusions they believe the original speakers have drawn, exactly as if they were their own. So why is it so much more worrying if I do it?

What I find more disturbing is that idiomatic translators not only operate with introspection but they often even leave the linguistic clues unconsidered that the original speaker has given in the Greek original as to how the text should be interpreted. These phenomena are exactly the phenomena I worked with in RRID, constraining devices that guide the hearer's processing, and show how the proposition is relevant in relation to other propositions and how it affects the overall representation of the hearer's world. Constraining devices are especially helpful in cases where the context is no more directly available because we know independently from context that the interpretation has to be constrained to a certain specific interpretation.

I do not agree with Hohulin that I did not present a research method. It is true I did not write a relevance discovery procedure book or a textbook with questions to answer, but then I would have hardly gotten my degree nor would the book have been printed by Cambridge University Press. I was under some constraints while I was writing the book and trying to get it published with a good publisher. In the meantime, however, together with a colleague, I have written a semantics and pragmatics course book for the German SIL course which is a bit more textbook-like and addresses matters of methodology in a bit more detail. Sorry, it is only available in German at the moment. In RRID I did, however, indicate clearly what my practical research was based on—tracing the hearers path in utterance interpretation. I also explained clearly how that works. In a nutshell this is what I said:

Utterance interpretation

1. Make a hypothesis about the intended proposition expressed, and enrich it by pragmatic means.

2. Chose a context in which the proposition is to be processed. (I explained in detail how that is done.)

3. Apply deductive rules.

4. Look out for contextual effects that the interpretation of the processing in that context has. The contextual effects could be of three kinds:
   Contextual implication
   Strengthening of existing assumptions
   Weakening or eliminating of an assumption.

5. Look for structural phenomena that constrain the hearer's processing to access particular contexts or to achieve particular contextual effect. (I focused very much on those.)

6. Consider least effort. Achieving contextual effect is achieving relevance. However, relevance is not only defined by contextual effect but also by 'least effort' in processing. (I explained how

structural phenomena may be explained by the difference in processing effort.)

Argumentation

Hohulin continues that she feels I have mentioned some other linguists' views in no great detail and that there is a suspicion that I did not sufficiently study notions with which I agree or disagree within the framework in which they were proposed. Spelling out other people's views in greater detail would have meant having to use more book space and that was already a problem. I had to reduce my original draft quite a bit.

As far as my knowledge of other theories is concerned, I cannot quite accept her criticism. I am not claiming that I am an expert in them, but I do believe that what I agreed and disagreed upon I understood well enough in order to have grounds for doing so. By the time my dissertation had been approved for publication a number of well-known linguists representing a number of different theories had read my book, edited it, and made suggestions for change. None of them actually said that I had not understood notions of other people's theories with which I disagreed. Anyway if they had, I would have expected them to give me particular examples, as it is usual when a claim of that sort is made. Unfortunately Hohulin does not tell me what exactly I might not have understood.

As far as my supposed argumentation against a linguistic approach to discourse analysis is concerned, I have to make a minor correction. I am against a PURELY linguistic approach to discourse analysis. All my particles are linguistic phenomena which I dealt with in a pragmatic way. Therefore 'pragmatic' always deals with language phenomena interpreted in context. I also usually gave at least a brief indication of their grammatical position.

I thought it was quite clear why I spent the first chapter arguing against a purely linguistic approach to discourse analysis, because from the beginning of the second chapter I began with a pragmatic approach.

The relation of the first two chapters to the data chapters has to be understood as the basis for my analysis—the relation between text and context and not between aspects of text. That in fact is what combines all the chapters and made the book of interest for CUP. It is a radically different notion from what other discourse analysts claim.

Chomsky and others have often introduced a major theoretical claim in their works, and then they have applied the claim to examples of perhaps a relative clause or coordinate structure. At this point it would not have been relevant to introduce rival theories of these structural analyses. The main point is giving examples for the major claim. This is also why I did not make rival theories for my particle analysis a main issue.

I think I have perhaps even gone a step further than just giving a few examples and, admittedly, I did that with SIL in mind. The detailed Sissala particle analysis was much less of interest to the linguistic world and CUP than I imagined it would be for SIL. Some reactions from within SIL have confirmed that my predictions were right. I have mainly gained respect for the particle analysis, although I wished that the major point that I have tried to make—that discourse connectivity is based on relations between text and context and NOT between items of text—would also be appreciated and taken as a basis for general SIL analyses. If it is the data analysis alone, without theoretical backbone that is seen as interesting, then the analysis can easily become a taxonomic one, such as grouping particles according to labels such as 'confirmatory' or 'parallel relation'. If the cognitive reasons for these labels are not understood then the taxonomy tells only a small part of the real story that those particles have to tell.

Conclusion

Hohulin's conclusion is in some respect very flattering. I am especially glad that she was able to find some usefulness in the theory for possible translation procedures. At the same time I am a bit surprised, if not disappointed, that she did not mention more concrete examples from my particular analyses for this. My book was mainly about discourse and particles and Hohulin draws no particular

conclusion from my analysis for translation checking, and yet I think she could have.

Translation consultants, with assumed knowledge in NT Greek, could for instance have found out that Koiné Greek, unlike in English, has two interpretations of *kai* 'also', only one of which can be used in English, just as I analyzed them with the Sissala and German data. They could have discovered a number of different parallel uses logically similar to my analysis. They could have noticed that some Greek particles make a big contribution to contextual fact, particles such as *gar, kai, de* and *alla.*

The Pauline letters are full of these particles and tracing their use is tracing Paul's communicative intentions. He not infrequently presents propositions which he wants to be understood as premises. Sometimes he mentions the conclusion in the text, introduced with particles such as *oun*, then he strengthens these conclusions, marking the backwards confirmatory proposition with *gar.* Sometimes he assumes his hearers to have certain assumptions which he wants to deny. He marks his denial of expectation with *de* or *alla.* When he uses *alla* he often confirms an explicit denial because an assumption has to be very strong in order to be eliminated. He may use *kai* to indicate a parallel relation of two propositions for various purposes, such as the parallel confirmation of a conclusion prior mentioned.

Of course these particles are an excellent means of translation checking—at least the discourse coherence in the target language. In fact, tracing them provides a clear and theory-based semantic-pragmatic structure analysis of the biblical text where the relations are not only based on intuitively labelled relations. Once one has understood what the basic constraining function of the particles is, a lot of other interpretations are ruled out. Since the original is constrained, the target language should not exhibit any other than the interpretation the Greek particles indicate. (For more detail on the Greek particles, see *Constraints on relevance in Koiné Greek* (Blass, forthcoming).

I would like to end on a more positive note on Lou Hohulin's efforts and express my great appreciation for her attempt to understand my book and comment on it in an unprejudiced way. ■

REVIEWS OF BOOKS

Theoretical issues in sign language research. Vol. 1: Linguistics.
Edited by Susan D. Fischer and Patricia Siple.
Chicago: University of Chicago Press, 1990. 334 pp.
Hardcover $55.00; paperback $29.95.

Reviewed by Andy Eatough

This volume is a collection of papers on various linguistic topics relating to sign languages. Most of the papers focus on American Sign Language (ASL), though some of them discuss data from other sign languages, including Swedish Sign Language, Taiwan Sign Language, Brazilian Cities Sign Language, Chinese Sign Language, and New Zealand Sign Language.

Although the title of the volume implies that all the papers discuss linguistic theory, some are more theoretical than others, and some are primarily descriptive. The first three deal with theoretical issues that come up in attempting to apply autosegmental phonology to ASL. The next two deal with phonetic questions, the application of the notions of the syllable and stress to ASL. Two chapters discuss verbs of motion and are fairly descriptive and pretheoretical. One chapter deals with deictic pronouns in Swedish Sign Language, and two deal with deictic pronouns in ASL. One article describes auxiliaries in Taiwan Sign Language, and one discusses modals in Brazilian Cities Sign Language. The last two articles in the volume discuss derivational morphology in Chinese Sign Language and New Zealand Sign Language respectively.

The first chapter, 'Temporal Aspects and ASL Phonology' by Wendy Sandler, proposes a canonical word structure for ASL signs in which there is only one hand configuration autosegment for each location-movement-location sequence, assuming an autosegmental phonology in which the elements that make up the timing tier are movements and locations. This departs from earlier autosegmental analyses of ASL (Liddell and Johnson 1984, Liddell and Johnson 1986, Liddell and Johnson 1989) in two respects.

In Liddell and Johnson's work since 1984, the timing tier had consisted of movements and holds rather than movements and locations. It is not clear to me exactly what Liddell and Johnson meant by 'hold', though Sandler seems to assume they meant something like 'a location at which there is a lack of movement'. If this assessment of Liddell and Johnson's meaning is correct, then Sandler's notion of a location is more general than Liddell and Johnson's notion of a hold.

The primary empirical advantage to using locations rather than holds as elements of the timing tier is that holds show up phonetically primarily in the 'list pronunciation' of signs, at the beginning and/or end of a sign articulated in isolation. This means that Liddell and Johnson need to either have rules to delete holds everywhere except utterance initially or finally, or else say that a 'hold' is pretty much the same thing that Sandler means by a 'location', and that holds do not necessarily involve any lack of movement in their articulation.

Far more interesting to me is Sandler's claim that ASL allows only one hand configuration autosegment for each location-movement-location sequence. She claims that all the apparent counterexamples in which there seem to be two hand configurations actually involve a single hand configuration and a movement internal to that hand configuration which crucially may not involve anything other than the opening or closing of a particular finger group. She points out that if two hand configuration autosegments are allowed in one sign, a large number of hand configuration sequences are predicted to exist which in fact do not exist. By claiming that hand configuration sequences within a simple sign do not exist in ASL, she rules out all the nonexistent sequences with a single constraint.

The difficulty that remains for her is to come up with an analysis of the sequences that do apparently exist which does not involve a sequence of hand configuration autosegments in the phonological representation. Since all the apparent sequences of hand configurations within a sign involve either the opening or closing of a single finger group, she posits a finger group autosegment which is attached to the hand configuration autosegment, and then has autosegments like **open** and **closed** attached to the finger group autosegment. This is analogous to Moira Yip's treatment of contour tones (Yip 1989).

The second chapter, 'Structures for Representing Handshape and Local Movement at the Phonemic Level' by Scott K. Liddell, attempts to eliminate some of the redundancies in Liddell and Johnson's earlier work, and also argues that the timing tier consists of movements and holds rather than movements and locations.

This chapter is largely a reply to Wendy Sandler's objections to Liddell and Johnson's movement hold model, and Liddell tries to show that there are empirical problems with Sandler's version of the timing tier. None of his arguments against the movement location model are very sound. He does manage to find one set of examples where Sandler's analysis doesn't work, but the real problem with her analysis of those examples is not her use of locations as elements of the timing tier, but rather her use of a phonological rule to insert a transitional movement. There are independent motivations for not including transitional movements in phonological representations at all, as argued in the next article by David Perlmutter.

The theory Liddell argues for in this article is somewhat different from the theory assumed in Liddell and Johnson (1986), since he proposes a large number of autosegmental tiers instead of the simple feature bundles the earlier theory had. However, he proposes far more autosegmental tiers than he motivates empirically.

Chapter 3, on the 'Segmental Representation of Transitional and Bidirectional Movements in ASL Phonology', by David M. Perlmutter, argues that transitional movements should not be considered part of the phonological representation of a sign at any stage in a derivation, and that bidirectional movements are geminates in which a single featural representation is associated with two movements on the timing tier.

Earlier work on the autosegmental phonology of ASL had assumed that transitional movements were to be inserted by phonological rules. Perlmutter points out that transitional movements occur precisely where the articulation of a sequence of signs would be physically impossible without a transitional movement. He argues that these transitional movements are a purely phonetic phenomenon, and need not be inserted by a phonological rule or be part of any phonological representation at any stage in its derivation. He presents several facts from ASL which are consistent with this view. In reduplicated signs involving a brush or rub, there is no brush or rub during the transitional movement. This is to be expected if the transitional movement is not in the phonological representation, since the brush or rub autosegment would not be expected to spread to an element that is not part of the phonological representation. Also, signers do not count transitional movements when counting beats in signing.

Perlmutter argues that bidirectional movements should be represented as geminate movements. In a bidirectional movement, it does not matter which direction the dominant hand goes first, as long as it goes in the opposite direction next. Thus, he argues, the direction of movement need not be part of the phonological representation. Everything about the two consecutive movements except their direction is always the same, including whether or not there is a brush or rub involved in the movement. Two consecutive movement elements on the timing tier which are unspecified for direction and which are both linked to the same articulatory features would be expected to behave in this way.

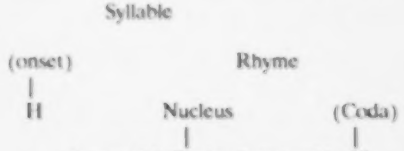
Perlmutter's chapter is the best argued of the theoretical chapters in this volume. It is concise and does not attempt to make more theoretical points than are motivated by the data under discussion.

The fourth chapter, 'Why Syllables? What the Notion Means for ASL Research' by Ronnie Wilbur, is a discussion of what the notion of the syllable might mean phonetically for ASL. Previous attempts at defining the syllable phonetically for spoken languages are carefully discussed. He points out that no phonetic definition of the syllable for spoken languages has yet been generally agreed upon, and then

shows that it would also be difficult to define the syllable phonetically for signed languages. The syllable has nevertheless been a very useful notion for spoken language phonology, and might also prove to be a useful notion for signed language phonology. It might at some point prove useful to state phonological rules for signed languages in terms of syllables, and if the spoken language phonologists can get away with not having an agreed upon phonetic definition for the syllable, perhaps the signed language phonologists could do without such a definition as well. It occurs to me that Sandler's constraint from Chapter 1 which required that there be only one hand configuration autosegment per location-movement-location sequence could be stated in terms of the syllable, by saying that there may be only one hand configuration autosegment per syllable in ASL. In Mexican Sign Language, there is a class of nouns which are normally reduplicated unless they have a suffix. This generalization could be stated in terms of the syllable also by saying that Mexican Sign Language has a preference for two-syllable nouns, or perhaps that it prefers words that are one metrical foot, if metrical feet could be motivated somehow. Not all of these signs actually contain a movement, and so the reduplication rule itself is perhaps better stated in terms of the syllable than in terms of location-movement-location sequences. To my knowledge no one has yet successfully shown that stress patterns are a significant part of the phonology of sign languages, but if stress ever is shown to be phonologically significant, then probably stress rules would be best stated in terms of the syllable.

Wilbur discusses two phonological phenomena which he argues are difficult to characterize without the notion of the syllable, constraints on when metathesis is permissible and the difference in unidirectional and bidirectional signs under reduplication.

He also suggests a model of the syllable for signed languages:



Wilber does not motivate every aspect of this model with phonological rules that make reference to the different parts. That would be a difficult thing to do, since it is not even entirely clear that the notion of the syllable itself is useful for signed language phonology. But Wilber's main point, that phonologists should look for ways that the syllable might be a useful notion in signed language phonology, is well taken.

Chapter 5, 'Emphatic Stress in ASL' by Geoffrey R. Coulter, asks what are the phonetic characteristics of stress in ASL. This is a relevant question, since as Coulter points out, signed languages lack distinctions of amplitude and pitch. There are apparently no metrical stress patterns in ASL, so research on ASL stress has focussed on emphatic stress. Coulter argues that in previous research on the phonetic correlates of emphatic stress in ASL, the effects of stress were confounded with the effects of other things. Significantly, he shows that Friedman's testing procedure made it difficult to distinguish the effects of stress from the effects of list pronunciation.

Coulter describes his own testing procedure, especially designed to avoid the problems inherent in Friedman's testing procedure, and then gives his results. He found that emphatically stressed signs were usually shorter in duration than unstressed signs, and that the movements tended to be larger. He predicts that future study will show that the phonetic effects of stress in signed languages are predictable from the greater amount of articulatory effort he assumes is involved. He bases this on the fact that the phonetic correlates of stress in spoken languages generally have some connection with greater articulatory effort. I can certainly see how articulating a sign with a larger than normal movement and a greater than normal speed would require greater than normal articulatory effort.

Chapter 6, 'Serial Verbs of Motion in ASL' by Ted Supalla, points out that verbs of motion in ASL are used in constructions which follow the same sorts of constraints that serial verb constructions follow cross-linguisticly. Properties of serial verb structures which Supalla takes from Sereechareonsatit (1984) are the following:

1. Series of verbs with only one subject
2. Series cannot be interrupted
3. Inflections apply to the whole struc[illegible]

4. Verb 2 tends to be reduced in form compared with an independent, nonserial verb

Supalla identifies morphemes in ASL verbs of motion for the following categories:

1. body part manner
2. path
3. manner along the path
4. direction of path
5. class of object undergoing the motion

Certain verbs of motion, namely those with body classifiers (verbs where the signer's body acts as a classifier), do not allow path morphemes. Other verbs of motion allow path morphemes but not body part manner morphemes. Supalla claims that each classifier subsystem used with verbs of motion has its own particular subset of the morphemes listed above that it may combine with.

He discusses constructions consisting of two verbs of motion, where some of the morphology is found on the first verb, and some of the morphology is found on the second. He argues that such constructions in ASL fit in with Sereechareonsatit's characterization of serial verb constructions. They are a series of verbs with only one subject. The series is never interrupted by anything. Inflections apply to the whole structure, if one accepts the idea that motion verbs in ASL are inflected for the categories that Supalla argues for, since for each of those categories, only one of the verbs will be morphologically marked. He claims that this construction also fits in with Sereechareonsatit's fourth point, in that the classifier for the second verb is 'relatively unmarked'.

This chapter suffers from vagueness and a lack of careful argumentation, but contains some interesting ideas.

Chapter 7, 'Predicates of Perceived Motion in ASL' by Ceil Lucas and Clayton Valli, is more descriptive than theoretical. They discuss a class of ASL verbs which previously had not been carefully studied in the literature. First they try to determine the productivity of the morphology that goes into each verb in this class, looking separately

at the productivity of the handshape, orientation, movement, and location parameters into which they divide the morphology of these verbs. They conclude that the morphology deriving these verbs is highly productive. Then they look in more detail at the location parameter and conclude that there are two different level systems in ASL morphology, one for representing the location of objects and events, and one for representing the perspective of the signer on an event. It is the second level system that figures in the class of verbs described in this chapter.

The scope of this chapter is limited enough that the subject matter can be treated well. The illustrations, drawn by Paul M. Setzer, are quite good, which is always a helpful feature in descriptive work on sign languages.

Chapter 8, 'Deictic Pronouns in Swedish and Swedish Sign Language' by Inger Ahlgren, is a comparison of the pronominal systems in Swedish and Swedish Sign Language. Her main point is that the pronominal systems of spoken languages like Swedish rely on the categories of person for distinguishing referents, and that Swedish Sign Language does not. In spoken languages, there is generally a category of first person for reference to the speaker or reference including the speaker, a category of second person for reference to the addressee or including the addressee, and a category of third person for reference to entities or groups that do not include either the speaker or the addressee. Ahlgren argues that in Swedish Sign Language the categories of person based on conversational roles are not relevant categories, and that the pronominal system distinguishes reference entirely by indicating location rather than by indicating conversational roles.

I was not very well convinced by Ahlgren's arguments. It is always much more difficult to show the lack of a distinction in the grammar of a language than to show the presence of a distinction. Although Ahlgren shows that location of referents is important for indicating reference in the Swedish Sign Language pronominal system, this fact does not in any way require us to conclude that person is not a relevant category. As a case in point, both location of a referent and the categories of person are relevant to the Mexican Sign Language pronominal system. Indicating the location of a referent is the primary way of distinguishing reference in the MSL pronominal

system, but person is a relevant category in other ways. Verbs without overt subjects have a default understood first person subject. Also, first and second person object pronouns behave more or less like French object clitics with respect to word order, and third person object pronouns do not. Thus both location and person can be relevant categories in the same language and the same pronominal system.

In fact Chapter 9, 'Person Deixis in American Sign Language' by Richard P. Meier, shows that a first person vs. non-first person distinction is relevant in the ASL pronominal system. There are idiosyncratic first person plural forms, whereas one would expect in a system where only direction of referents and not their person was relevant to have first person plural forms that differed from other pronouns only in direction of orientation or movement. Meier also gives an argument from role playing for the same thing.

However, Meier argues that the person distinction in ASL is simply between first person and non-first person, and that second person is not a distinct category. He points out that the phonological form of a pronoun that refers to the addressee does not in any way depend on that fact of that person's being the addressee, but depends entirely on the location of the addressee. He also says that eye gaze at the addressee cannot be claimed to be the phonological distinction between second and third person pronouns, since eye gaze is a property of conversations as a whole rather than of particular signs.

As with the previous chapter, claims based entirely on a lack of evidence to the contrary should be treated with extra caution. However, Meier seems to realize this, and makes the claim about the lack of a second person category with appropriate tentativeness.

Chapter 10, 'Pointing Out Differences: ASL Pronouns in Syntactic Theory' by Diane Lillo-Martin and Edward S. Klima, argues against Meier that there is no person distinction with ASL pronouns, and that in fact there is only one pronoun in the lexicon. They ignore possessive pronouns. They also ignore the idiosyncratic first person plural pronouns mentioned by Meier and deal only with his arguments from role-playing. They argue on the basis of a variety of facts from spoken languages that it is necessary crosslinguistically to distinguish referential indexing from actual reference, and that this

distinction allows them to account for Meier's role-playing facts without positing more than one pronoun or having a person distinction in their analysis of ASL.

Chapter 11, 'Evidence for Auxiliaries in Taiwan Sign Language' by Wayne H. Smith, is to my knowledge the first place in the literature where a sign language has been claimed to have auxiliaries. There are a number of verbs showing agreement directionally that combine with a verb that does not exhibit agreement, and Smith says this is the same sort of thing as when spoken language auxiliaries bearing agreement morphology combine with infinitives that lack agreement morphology. What is striking is that when these auxiliaries combine with a verb that normally does show agreement directionally, the second verb does not show the agreement that it normally would, and all agreement is indicated by the auxiliary.

This chapter is a good piece of descriptive work. The illustrations are cartoon-like, but entirely adequate for presenting the data.

Chapter 12, 'Epistemic, Alethic, and Deontic Modalities in a Brazilian Sign Language' by Lucinda Ferreira Brito, is by far the most difficult to follow of the articles in this volume. I would recommend that most readers skip the text of the article and just look at the illustrations, which were rather nicely drawn by Ester and Antonio Arruda.

She discusses the interrelationships of the epistemic modality, the alethic modality, and the deontic modality. The epistemic modality (indicated in English by **might** or **possibly**, etc.) is where the truth conditions of a modalized clause are met as long as the propositional content of the clause is true in some possible world(s). The alethic modality (indicated by **necessarily must** in English) is where the truth conditions of a modalized clause are met just so long as the propositional content of the clause is true in all possible worlds. The deontic modality (indicated by **should** or **ought to** in English) is where the truth conditions of a modalized clause are met just so long as the propositional content of the clause is true in the 'best' of possible worlds, or in those possible worlds where the moral and ethical obligations of the subject are met. She describes how the various modalities are indicated in Brazilian Cities Sign Language, and then begins a long discussion which seems to center on the

question of what sort of diagram is best suited for illustrating the interrelationships of the modalities indicated by Brazilian Cities Sign Language modals.

I think I understand a few of her claims well enough to try repeating them here. She claims that there is no alethic modality system in natural languages, and that the modals that are traditionally considered alethic (**must**) actually are part of the epistemic modality system, which can be considered a continuum with regards to degree of certainty expressed by different modals. This epistemic continuum would be isomorphic to a deontic continuum of degrees of obligation. Expressions like **necessarily must** would be at one end of the epistemic continuum, and modals like **might just possibly** would be at the other end of the continuum. It is not clear to me what empirical predictions this claim makes, or whether it makes any empirical predictions at all. It seems to be a relevant question only for deciding what kind of diagram is best for illustrating interrelationships of natural language modals in terms of the parameters **degree of certainty** and **degree of obligation**.

Chapter 13, 'Lexical Branching in Sign Language' by Shunchiu Yau, is a discussion of productive nonconcatenative derivational morphology in sign languages. He is concerned with several different formational parameters that might conceivably be modified to derive a new sign from an existing sign: hand configuration (shape), hand orientation, location, movement, and facial expression. He compares American Sign Language, Chinese Sign Language, and two isolate sign languages to see which strategies are most often used. The strategy of changing the hand configuration and leaving the rest the same turns out to be far more common in ASL than in the other sign languages considered, and Yau points out that initialization is unlikely to occur unless there is a system of fingerspelling in common use. Chinese Sign Language, which does not make frequent use of fingerspelling or initialization, more often derives new signs by changing the movement parameter of an existing sign. The two sign language isolates appear to make some use of all the different parameters Yau considers, though the few signs that involve a change on the hand configuration parameter are not initialized signs like the ASL examples.

Chapter 14, 'Word Formation Processes in New Zealand Sign Language' by Marianne Collins-Ahlgren, is primarily a descriptive article on productive derivational morphology in New Zealand Sign Language. It starts out with a review of the theoretical literature on the phonology and morphology of sign languages. The article is organized partly around a statement from Hockett (1958) which Collins-Ahlgren cites in her introduction, to the effect that '...duality, arbitrariness, productivity, and cultural transmission...' are distinguishing properties of human language. I would have liked to have seen in the introduction an explanation of what Hockett meant by this.

The articles in this volume almost without exception would be worth reading for any linguist who wants a quick and fairly painless introduction to what is going on in sign language linguistics. Some basic knowledge of American Sign Language would be helpful but not necessary before reading them. I would also recommend first reading Padden (1983) or some other extensive grammatical description of a sign language.

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Language repertoires and state construction in Africa: Cambridge studies in comparative politics. By David D. Laitin. Cambridge University Press. 1992. Pp. 176. £30, $42.95.

Reviewed by Genevieve M. Hibbs, Ph.D., (a contribution of Wycliffe Associates, U.K.)

Using African countries as exemplars, which languages are likely to be promoted and which be left to die; what are the macro and micro forces which determine language use; how can those forces be understood? Laitin's book answers these questions in an interesting and plausible way utilizing game theory. He looks at language repertoires and the processes which have brought these about in particular modern states, and classifies the results associating African and non-African examples as follows: language rationalization, 2-language outcome, and 3 $\pm$ 1 language outcome. Nevertheless, the routes by which these outcomes are achieved diverge, and the reasons for those divergencies are also explored.

Anyone who is involved in policy making that will affect language choice and language facilities availability in specific geographic areas, or to specific groups of people, ought to be aware of the types of forces and interests that are likely to operate in various types of political and social groupings with their unique political, social and language backgrounds. Laitin's book sets out many of the forces and interest types involved in a usable, interesting and readable way.

Just before receiving the copy of Laitin's book I had been listening to a BBC Radio 4 report which explained how one natural language in Yugoslavia had been deliberately made as different as possible from its parent, by the users, to the extent of using multiword functional descriptions rather than single words like **auto** for **car**. It stimulated a pattern of enquiry to which Laitin's book provides some of the answers.

The current context of the disintegration of Eastern Europe and the Baltic States, and the withdrawal of sponsorship from third world countries, with all the human cost involved, point to the need to reconsider the 'value of war making (as) a significant ingredient in the development of the administrative capacity to ensure monopoly of

violence in a large territorial area, the distinguishing feature of a modern state' (Laitin xi), with all its implications for language use. What should we be promoting, praying for; is monopoly of violence which then doesn't have to be applied, after all, a better situation?

It is unfortunate that Laitin chooses to dismiss cybernetic theory as if it had no relevance on the basis of one application, i.e. communication networks with individuals as nodes, by Deutsch (27,30). If he had applied cybernetics as the prime multidisciplinary discipline, along with the game theory, the result could have been even more illuminating. Laitin's interpretation of his matrices was obscure. I was completely confused by the verbal descriptions of the cells (North East, South West, etc. and choices 1 to 4) which were not labelled in the charts themselves. Cybernetic theory could have been applied there.

The lack of glossary was noted in relation to the use of **vehicular** when talking of Swahili and languages of the Congo. 'Vehicular languages or lingua francas' (87-88) seems to require a little more explanation in this technical context. The definitions for four other concepts are indexed, which is a suitable way of dealing with the matter as long as the enquiring reader locates them there. Cybernetically, tables of contents, for example, could be used creatively to point from absent components, like from a glossary, to an index in which definitions are specifically highlighted.

It is also unfortunate that Laitin has not taken a little more trouble in his research about the Summer Institute of Linguistics (SIL) (98-99). Many countries contribute to the work of SIL providing both translators and their support. It is by no means solely a U.S. mission and the unnecessary statement with 'most of its missionaries' being 'whites from the Midwest and South' is to ignore its international and multiethnic composition. *Community literacy programmes in Northern Ghana* (Bendor-Samuel 1983) which Laitin quotes from, its title clearly shows there is a wider sense of responsibility within SIL than merely name a language, translate its Bible and then leave! Host countries' authorities are usually involved at all crucial stages in the language decisions being made.

Centers teaching social linguistics should have a copy of Laitin's book for reference and encourage students to select readings from it.

Linguistic consultants should be encouraged to read it, and some, especially those concerned with Africa or African languages, will want to keep a copy for reference.

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Prolog and natural-language analysis. By Fernando C. N. Pereira and Stuart M. Shieber. CSLI Lecture Notes No. 10. Menlo Park, CA: Center for the study of language and information. 1987. Pp. 266. Cloth $28.95, Paperback $13.95.

Reviewed by Thomas M. Tehan

Payap University (Chiang Mai, Thailand) and SIL Thailand.

Welcome to the thick of computational linguistics—at least for us OWLS. Perhaps you've wondered if it is possible to do a little or a lot more linguistics with your computer. As you dive into this book, you will begin to get an idea of length and depth of the intersection of computers and linguistics. This will particularly be so for many of us, since the linguistics that Pereira and Sheiber do with the computer is not using a new alphabet for the ED editor, or a database like Shoebox, or a Text Analysis that produces word or phoneme lists.[1]

The book presents an introduction to the graduate school level of computational linguistics and a tour of various subsets of Prolog: database Prolog, pure Prolog, and full Prolog.

The authors state in the Introduction:

> This book presupposes some acquaintance with elementary notions from logic, formal-language theory, computer science and linguistics (p. 6).

The Bibliographic Notes at the end of the Introduction suggest several books that can be read to acquire these 'prerequisites' to gain maximum benefit from this book. In fact, if you don't feel qualified to read this book yet but would like to become qualified, the study of these recommended books can help to qualify you and justify your purchase of the book.

The authors' statement of their thesis or central idea is as follows:

> This book is an introduction to elementary computational linguistics from the point of view of logic programming ... The main goal of the book is to enable the reader to acquire, as quickly as possible, a working understanding of basic computational linguistic and logic programming concepts (p. 1).

If these concepts are new and challenging to you, I hope that this review can serve as quick introduction to the field of computational linguistics. By the end of the review you should know if this book is the right one for you to begin with.

The Content and Ideas of the Book

The authors gradually present more complex examples of a grammar that is able to parse English sentences and produce them to some extent. The work is marked by clear, logical and consistent organization. The concepts and terminology are precise, and thus the reading becomes more and more demanding as the reader works his way through.

There are several key concepts and technical terms that are defined in the text. The reader of this review can benefit from understanding these terms and perhaps appreciate the scope and complexity of the book from these terms, all discussed within the introduction of the book.

Computational linguistics is the study and application of material that lies at the intersection of computer science and linguistics. It's ultimate goal it not just for the computer to be programmed to do linguistic analysis but for the computer to be able to use language in a way that is analogous to human use of language.

A **database** is a set of information organized in a way that facilitates retrieval of any or all of that information in response to queries, i.e. questions.

Prolog as a name is derived from the words 'PROgramming' and 'LOGic'. Prolog is a computational programming language that enables basic symbolic logic to be represented rather directly. It thus turns out to be a suitable tool for implementing 'basic components of natural-language-processing systems' (p. 1). 'Logic programming' is a set of programming and interpretation rules of inference with a procedure to apply those rules systematically on the computer. To work effectively, it must be able to search completely through all possibilities in the database of knowledge and return logical answers to computer inquiries.

Declarative and procedural languages are designed for computer programming. Prolog is declarative, i.e., it states what it wants done regardless of the method for doing it. It is up to the computer to figure out how to accomplish the tasks. In contrast, C, Fortran, COBOL, Pascal, BASIC, etc. are procedural, i.e., they instruct the computer how to compute and present the desired results. They must exactly and minutely tell the computer how to accomplish the tasks.

'**Semantic networks** are graph structures often used for knowledge representation in artificial intelligence' (p. 26). They closely resemble the schemas that cognitive psychologists use to represent the interconnected patterns of our human minds. They are sometimes used in relation to SIL translation theory.

'**Context-free grammars** (CFG) constitute a system for defining the expressions of a language in terms of rules, which are recursive equations over expression types, called nonterminals, and primitive expressions, called terminals' (p. 29). For example, 'S → NP VP' states that a sentence S can be replaced with NP and VP in all contexts, thus the name 'context-free'. Their importance springs from the ease with which CFGs can be encoded into Prolog statements.

Organization of the Book

The tone and style of the book is not really too formal, but the subject matter quickly becomes complex and detailed. This is not an

introductory survey of computational linguistics, and it is not an easy, chatty, build-confidence-while-you-use-it instruction manual in Prolog programming.

After the first couple of chapters, the reader will have a hard time just skimming the book without working through the examples in detail. The reader must master the content of each chapter before he moves on to the next one. Each chapter is coherent.

The book is divided into six chapters, which are not grouped into sections or parts. The chapter titles reflect the progression of the book's discussion from a 'Database Prolog' (chapter 2), through 'Pure Prolog' (3), to 'Full Prolog' (5), with chapters entitled 'Introduction' (1), 'Further Topics in Natural-Language Analysis' (4), and 'Interpreters' (6) also included.

There are no tables or illustrations to break up the text, but there are a few parsing trees and schema diagrams. There are also many Prolog program listings. There are three appendices that fall under the overall heading of 'Listing of Sample Programs':

1) A Note on Programming Style
2) The TALK Program, and
3) The DCG Compiler

The last two are sample programs; the first appendix is two pages of advice on programming. There are two indexes, the first organized by name and the second by subject.

Exercises are interspersed throughout the text and are intended to help readers verify their understanding of the concepts covered. They are progressively more challenging and complex the further the reader goes in the text. The later exercises build on earlier ones. Be prepared to do most of the exercises or you won't be able to handle the detail of later chapters.

The detailed bibliography is 13 pages long; in addition, there is a six-page listing of CSLI publications. Entries from other countries are well represented. The bulk of the bibliographic entries are from the 1970s and 1980s, although there are some from the 1950's and 1960's. There seems to be a representative balance of books and journal articles.

The annotated bibliographic notes at the end of each chapter are great. They provide succinct comments on books that open the doors to many new aspects of computational linguistics. These notes follow the order of topics presented through the respective chapters.

Evaluation of the Book

When is this book valuable? If you know linguistics (especially syntax), and possess some computer programming skills, and can handle symbolic logic at some introductory level, then this could be a book for you. If you now want to tie those together into more than a cursory introduction to computational linguistics, then again this is a good book for you. But expect a challenge and some hard thinking if your computer skills are moderate.

While it is possible for a person to learn and profitably use Prolog without a knowledge of the symbolic notations of first-order logic (FOL), this is not the book for that person to learn Prolog. It's tough going if you don't know FOL and haven't done some computer programming. This book teaches computational linguistics based on FOL using Prolog. It demands the reader follow mathematical proofs in FOL. If you feel a novice in computer programming and symbolic logic, then perhaps other books might be more helpful.

For an introduction to First-Order Logic, Barwise and Etchemendy's *The Language of First-Order Logic* is both readable and helpful. If you just want to learn some Prolog and play around with it a while, then study a Prolog textbook like Sterling and Shapiro's *The Art of Prolog* or the introductory manuals to a PC Prolog package like Borland's *Turbo Prolog*. They would be better general introductions to Prolog. If you just want a glimpse into computational linguistics, then the first few chapters of Ralph Grishman's *Computational Linguistics* would be a better general introduction to computational linguistics. If you want about an hour's reading for introduction to computational linguistics, go to a broad based introductory linguistics book with a chapter on computational linguistics. For a good one chapter introduction see Judith Klavans, *Computational Linguistics*.

However, if you are comfortable reading about topics such as the scope of variables and existential quantification, this book is a good meaty introduction to several aspects of computational linguistics.

And if you want to integrate Prolog programming and computational linguistics in a fair amount of depth, this book will meet your expectations.

Note

[1]All of these programs are available from SIL.

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